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The Financial Premium in the US Labor Market: A Distributional Analysis

Ken-Hou Lin, *University of Texas at Austin*

Using both cross-sectional and panel data, this article revisits the evolution of the financial premium between 1970 and 2011 with a distributional approach. I report that above-market compensation was present in the finance sector in the 1970s, but concentrated mostly at the bottom of the earnings distribution. The financial premium observed since the 1980s, however, is largely driven by excessive compensation at the top, a development that increasingly contributes to the national concentration of earnings. Furthermore, the analysis suggests that the financial premium for top earners remained robust in the early 2000s, when deregulation slowed down, and in the aftermath of the recent financial meltdown. These findings are inconsistent with the account that the earnings differential is driven by unobserved skill difference and demand shocks but supportive of the institutional account of rising inequality.

Excessive compensation in the finance sector has been well known since long before the crisis of 2008. For over a century, the *New York Times* has commented on Wall Street's annual bonuses. Its headlines range from how the size of the bonuses varies from the previous year and across firms to whether financiers will spend their checks on private jets, sports cars, or upgrading their apartments. Since the late 1980s, the best and the brightest (and perhaps the most privileged) of America's college graduates have been increasingly attracted to the finance sector for its lucrative rewards (Gerson 2009; Goldin and Katz 2008). Sources indicate that about one-third of recent Harvard, Princeton, and Yale graduates take their first job in finance, an industry that pays its average interns monthly wages of \$8,000 (Rampell 2011; Terkel 2011).

The prevailing explanation for such excessive compensation in labor economics is the increasing demand for intensive, skilled labor. Indeed, the Current Population Survey (CPS) shows that the finance sector houses a more educated workforce. In 2011, almost 57 percent of full-time workers in finance had at

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least a bachelor's degree, compared to 34 percent in the non-finance sector. Yet, recent studies (Philippon and Reshef 2012; Tomaskovic-Devey and Lin 2011) find that workforce characteristics alone do not account for the increasing compensation: financial workers on average earn significantly more than their counterparts in the non-finance sector, even when accounting for human capital or time-constant unobserved characteristics.

The case of the financial premium is particularly relevant to the study of inequality and stratification. Studies show that recent inequality growth in the United States is concentrated at the upper end of the wage distribution (Autor, Katz, and Kearney 2008; Lemieux 2008; Piketty and Saez 2006). Yet, little agreement has been reached as to the primary driving forces behind this development. While some scholars attribute this result to demand shifts induced by technological changes (Acemoglu 2002, 2003; Autor, Katz, and Kearney 2008), others offer political and institutional explanations (Hacker and Pierson 2010; Lemieux 2008; Lin and Tomaskovic-Devey 2013; Western and Rosenfeld 2011). The emergence of a financial premium provides an empirical opportunity to examine these competing explanations. Furthermore, recent studies (Bakija, Cole, and Heim 2012; Kaplan and Rauh 2010; Philippon and Reshef 2012; Tomaskovic-Devey and Lin 2011) suggest that excessive compensation in the finance sector plays a significant role in the concentration of income in the United States, while similar developments are also found in the UK and France (Bell and Van Reenen 2010; Godechot 2012).

This article extends previous studies (Freeman 2010; Philippon and Reshef 2012; Tomaskovic-Devey and Lin 2011) in three ways. First, previous studies focus mainly on the average financial premium of the entire sector and thus overlook how the financial premium might be distribution specific. This article examines the variation of the financial premium across time and, more importantly, across the earnings *distribution* with Re-Centered Influence Function regressions (RIFs). The analysis of workers between the 5th and 95th percentiles of earnings show that above-market compensation was present in the finance sector in the 1970s, but it was concentrated mostly at the bottom of the earnings distribution. As a result, the compensation structure of the finance sector actually reduced economy-wide inequality prior to the 1980s. The rise of the financial premium since the 1980s, in contrast, has been driven primarily by excessive compensation at the upper end. In addition to providing a more nuanced account for the earnings dynamics, examining the variation of the financial premium among workers provides insights into the potential causes of excessive earnings. The evidence suggests that, corresponding to the deepening political involvement and consolidation of the finance sector, a discontinuity in the premium emerged between elite and ordinary financial workers in the 1980s, a result that contradicts the market account of wage premium. In recent years, the premium for the 95th percentile is two times greater than that for the 90th percentile and five times greater than that for the median.

Second, scant existing research examines whether the financial premium persists in the post-crisis period. The analysis in this article indicates that the financial meltdown and the subsequent credit contraction had little effect on the financial premium. In fact, the financial premium received by elite financial workers

appears to be larger during 2009–2011 than during 2005–2008, though the difference is not statistically significant. This finding contradicts the account that the financial premium is driven by an increase in demand for financial services.

Last, though suggested in previous studies (e.g., Tomaskovic-Devey and Lin 2011), how excessive compensation in the financial sector widens the overall earnings distribution has never been formally assessed. This article estimates the impacts of the financial premium on growing inequality with a Oaxaca-type decomposition. The result shows that, indeed, the financial premium has been increasingly contributing to the divergence of earnings trajectories between the median and the upper end over time. The estimates suggest that more than 11 percent of the earnings growth at the 95th percentile since 1970 can be attributed solely to dynamics associated with the finance sector, which constitutes, at most, 5 percent of the US workforce. The financial premium alone accounts for more than 70 percent of this contribution.

This study advances the literature on the rise of finance and its inequality consequences in the United States (Bakija, Cole, and Heim 2012; Crotty 2003; Kus 2012; Lin and Tomaskovic-Devey 2013; Palley 2008; Philippon and Reshef 2012; Tomaskovic-Devey and Lin 2011) and other advanced countries (Bell and Van Reenen 2010; Godechot 2012). By examining how the financial premium varies across time and distribution, I provide a new and more complete picture of the compensation differential between the finance and non-finance sectors. Moreover, this study contributes to recent discussion on the sources of inequality (Acemoglu 2002; Autor, Katz, and Kearney 2008; McCall and Percheski 2010; Morris and Western 1999). Although many scholars attribute the rapid growth in inequality since the 1980s to skill-biased demand shocks, the findings here suggest that industry-specific political and institutional dynamics should be a target for investigation.

The Rise of the Financial Premium

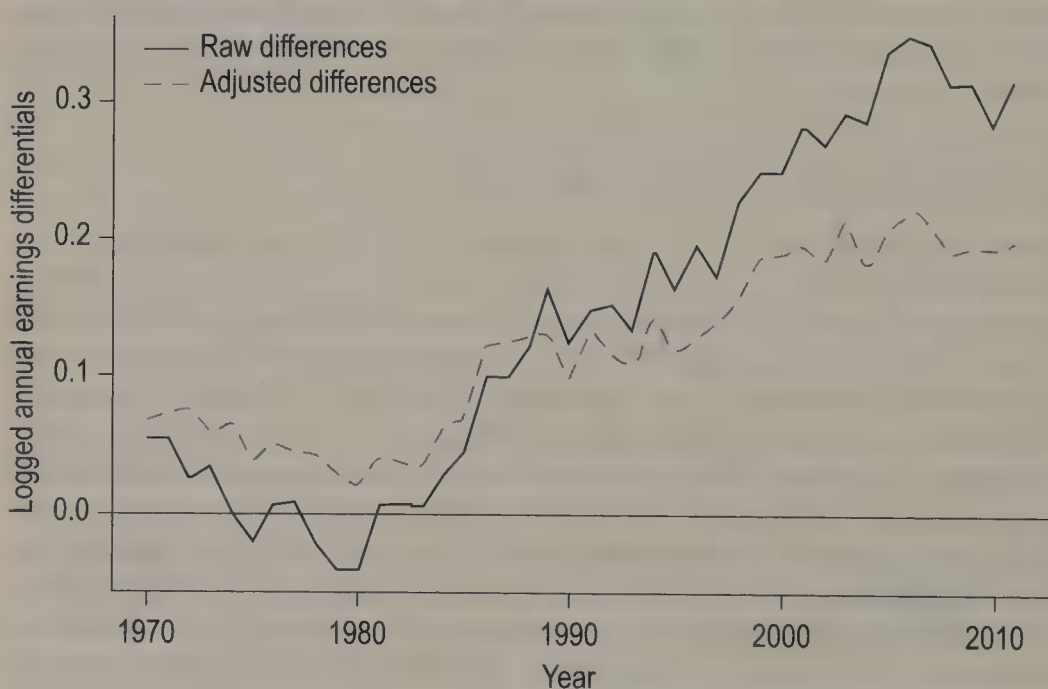
Excessive compensation in the finance sector is well documented. Sum et al. (2008) estimate that, in 2007, the average weekly wage in the investment bank and securities industry of Manhattan was six times higher than the average wage of workers in Manhattan and 20 times higher than that of workers elsewhere in the United States. Kaplan and Rauh (2010) find that an increasing fraction of top earners are investment bankers and hedge-fund managers. Similar results are presented in Bakija, Cole, and Heim's (2012) analysis of the occupational composition of all top earners. They find that the share of the top 1 percent that were in financial professions doubled from 7.7 percent in 1979 to 13.9 percent in 2005. Parallel trends are observed in other developed countries. Bell and Van Reenen (2010) show that in the UK, more than one-half of the growth of the top decile between 1998 and 2008 went to financial workers. Across the Channel, Godechot (2012) finds that finance is similarly responsible for the concentration of earnings since the late 1990s in France.

Philippon and Reshef (2012) provide an extensive analysis of the evolution of the US finance sector between 1909 and 2006. Their study shows that workers

in finance were relatively high skilled and well compensated before 1940 and after 1985 but not during the interim period. Their study also finds that the wage differential could not be explained by workforce characteristics, employment risk, or earnings profiles. Tomaskovic-Devey and Lin (2011) argue that the compensation differential represents economic rent, which also takes the form of corporate profits in the banking industry. They estimate that the total transfer into the finance sector between 1980 and 2008 could be as much as 6.6 trillion 2000 dollars.

Figure 1 presents the evolution of the unconditional and adjusted financial premium between 1970 and 2011. The solid trend line denotes the mean differences in logged annual earnings between the finance and non-finance sectors. This figure shows that the average earnings in the finance sector were similar to those in the non-finance sector in the 1970s. The difference emerged in the 1980s and expanded dramatically during the next three decades. By the end of the time series, workers in the finance sector, on average, earned 30 percent more than workers in other industries. The dashed trend line denotes the average differences adjusted for human capital, geographical, and supply-side characteristics. This line indicates that some financial premium was present in the 1970s, but the size was dwarfed by its later expansion. At its peak, workers in the finance sector earned 20 percent more than their roughly equivalent non-finance counterparts. The adjusted trend also suggests that the changes in the

Figure 1. Evolution of the financial premium, 1970–2011



Source: IPUMS-March CPS. The dependent variable is logged total annual wage and salary income. Top-coded values prior to 1995 are re-imputed with the top-code cell means from the restricted CPS data (Larrimore et al. 2008). The adjustment is made by year-specific OLS models. All control variables used for the adjustment are described in the variable section.

workforce composition account for only one-third of the financial premium in recent years.

More importantly, figure 1 shows that the growth of earnings differential in the 1980s is not explained by workforce composition. That is, the financial premium took off before any educational upgrade, which makes the latter a less convincing explanation for the rise of the financial premium. A more likely scenario is that the financial premium attracted highly skilled workers into the industry and in turn generated larger earnings differentials.

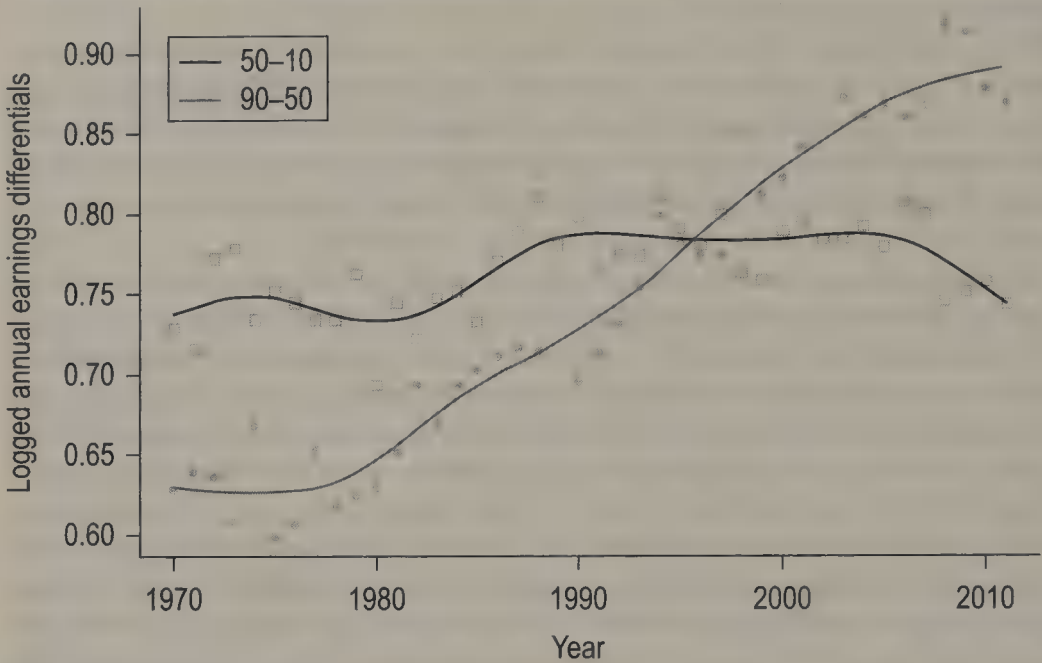
While the *average* earnings differential between the finance and non-finance sectors has been increasing, how the premium is distributed within the finance sector remains largely obscure. It could be that the premium is shared to a similar extent among financial workers. In other words, most workers in the finance sector might receive equivalent increases in their compensation. Freeman (2010) suspects, however, that the increase in compensation is disproportionately received by financial workers in higher positions, as inequality has increased more in finance than in other sectors. This scenario is supported by the finding that the CEOs of financial firms receive much greater premiums than average financial workers (Philippon and Reshef 2012) and that managers, professionals, and sales workers receive larger premiums than other financial workers (Tomaskovic-Devey and Lin 2011).

These discrepancies demand a distributional analysis on the nature of the financial premium. In the case that the financial premium is associated with the level of earnings, an emphasis on the average would overlook the differences among sets of workers. Furthermore, an exclusive focus on the average would obscure how the financial premium may shape the overall earnings inequality. As shown in the following analysis, the financial premium in fact gradually declined for low-paid workers but increased much more dramatically than the average for elites. As a result, the shift in financial premium from the bottom to the top has exacerbated the widening earnings inequality in the United States.

Growing Inequality

The rise of the financial premium coincided with a sharp growth of income inequality in the US labor market. The inequality growth was pervasive at first, but since the 1990s, it has been driven mostly by income dynamics in the upper half of the earnings distribution (Autor, Katz, and Kearney 2008; Lemieux 2008; Piketty and Saez 2006). Figure 2 reiterates previous findings. The solid line denotes the logged earnings gap between the 50th and 10th percentiles. It shows that inequality in the bottom half widened in the 1980s but plateaued or even declined in the late 2000s. The 90–50 gap, in contrast, had a steady 40 percent growth between 1980 and 2010. Over the past few years, the 90th percentile earned almost twice as much as the median.

Inequality growth at the bottom in the 1980s is commonly attributed to a decline in the real minimum wage (Lee 1999), but there is less agreement on the main driving forces behind the steady increase at the upper end. A variety of accounts have been developed to explain the concentration of earnings, and implicitly, to answer

Figure 2. Trends in earnings inequality, 1970–2011

Source: IPUMS-March CPS.

the question of whether it promotes economic growth. The prevailing explanation in labor economics is technological developments that disproportionately increased the productivity of skilled, non-routine workers, while weakening the demand for blue-collar, routine workers (Acemoglu 2002; Autor, Katz, and Kearney 2008; Autor, Levy, and Murnane 2003). This account is consistent with the polarization of the US labor market (Autor, Katz, and Kearney 2006; Kalleberg 2011) and increasing educational wage differentials (Goldin and Katz 2009; Mincer 1996). When examining the connection between workplace skills and compensation, however, Liu and Grusky (2013) find that the increasing payout to computer skills is less significant than those to either analytical or managerial skills. Furthermore, the skill-biased technological change account does not explain why technological change and inequality growth did not go hand in hand in such countries as France and Japan (Card and DiNardo 2002).

An expanding literature examines institutions such as unions (Card 1996; Freeman 1994; Western and Rosenfeld 2011), centralized wage-setting regimes (Tomaskovic-Devey et al. 2009), flexible employment contracts (DiPrete et al. 2006; Kalleberg 2011), benchmarking (DiPrete, Eirich, and Pittinsky 2010), performance-pay practices (Hanley 2011; Lemieux, MacLeod, and Parent 2009), and social norms in general (Piketty and Saez 2006; Western and Rosenfeld 2011). The consensus is that the effects of technological changes and demand shifts are strongly mediated by shifting labor-market institutions. This explains why similar economic changes affect the earnings distribution and employment stability differently across countries.

Hacker and Pierson (2010), in contrast, emphasize the government's decisive role in conditioning the markets. They argue that organized business interests

since the 1970s and “policy drift,” the failure to adjust policies according to environmental changes, are responsible for the winner-take-all economy. In particular, they identify taxation, industrial relations, executive compensation, and financial deregulation as four policy arenas in which the government fails to promote the public interest. This view is consistent with Volscho and Kelly’s (2012) findings that the surge of the top 1 percent’s income share is associated with right-ward shifts in Congress, deunionization, tax cuts for the top, trade openness, and asset bubbles.

Financial Premium and Inequality

The rising financial premium provides two ways to advance the understanding of the growing inequality in the United States. First, if the financial premium disproportionately concentrates among the top earners, it would partially account for the divergence in the upper half of the earnings distribution. I estimate the size of this contribution in the analysis below.

Second, the financial premium provides an empirical case to examine various claims on growing inequality. Most studies view the financial premium as tightly coupled with the supply and demand in the labor market. Kaplan and Rauh (2010) argue that there is no obvious connection between social norms and the rise of financial professionals and argue that financial workers’ excessive earnings is due to the interaction between scale and technological change. In this account, the advancement in information technology augments the relative productivity of highly skilled workers, which in turn leads to growing compensation differentials. Bell and Van Reenen (2010) hold a similar view, arguing that the financial premium is at least partially driven by talented individuals taking advantage of a more globalized financial market. As Bakija, Cole, and Heim (2012) point out, however, technological change does not explain why financial workers earn more than their equally skilled non-financial counterparts, unless technological development is also finance biased and disproportionately increases financial workers’ relative productivity.

Although operating under a similar assumption that the financial premium is driven by greater demand, Philippon and Reshef (2012) find technological change an unsatisfying explanation. Instead, they point to financial deregulation, the increasing complexity of corporate finance, and, to some extent, financial innovation as alternative explanations. They argue that financial deregulation increased the sector-wide demand for skilled labor, which led to increases in human capital and relative wages. Yet, as indicated in figure 1, the educational-earnings differential does not fully account for the premium in the finance sector. Furthermore, it does not explain why the compensation for financial workers did not start to return to equilibrium in the 2000s, when there was no further significant deregulation. The persistence of the financial premium over three decades is truly puzzling, if one views the US labor market as largely competitive.

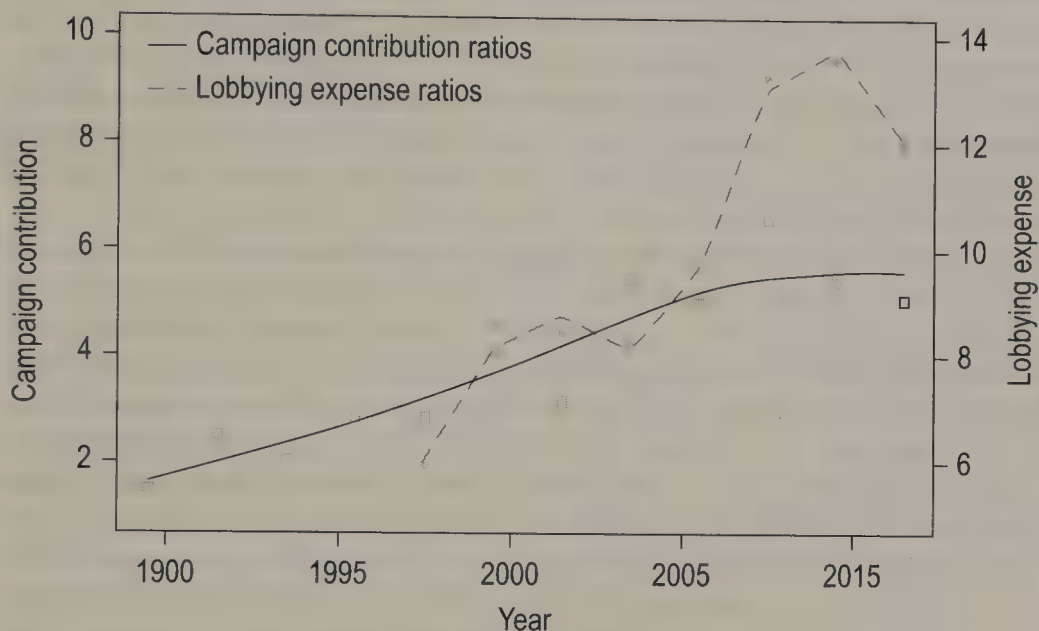
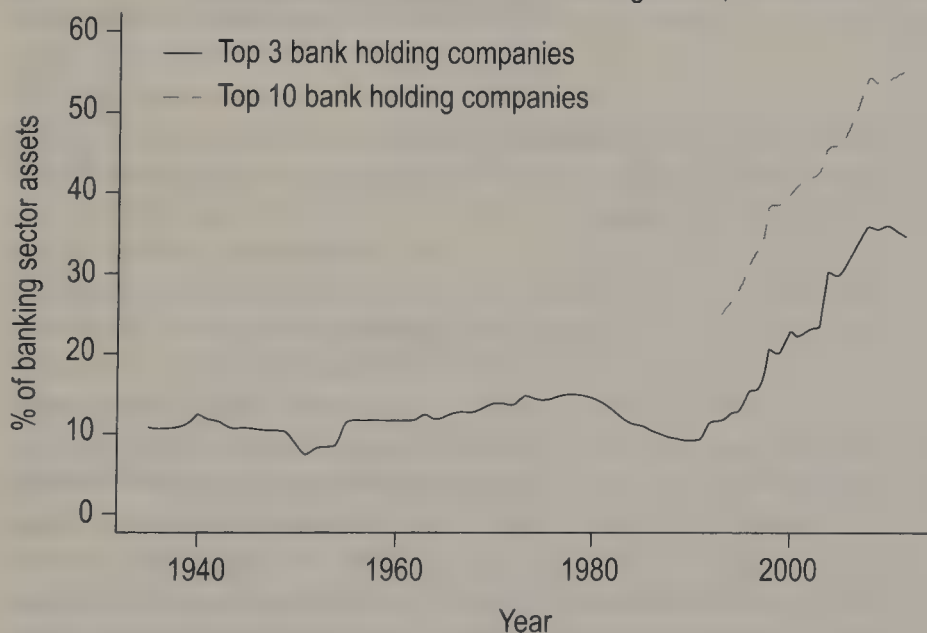
Tomaskovic-Devey and Lin (2011) trace the institutional roots that gave rise to financial deregulation and the explosion of financial activities since the late 1970s. They argue that the financial premium resulted from a series of political

decisions and business mobilization that reoriented the US economy toward financial markets. By this account, financial deregulation and the complexity of corporate finance were not exogenous shocks but results of interactions among the state, the finance sector, and non-financial firms, in which the finance sector constructed increasing economic rents and conditioned the market through regulatory capture, consolidation, shareholder activism, and the promotion of neoliberal economic models. In this light, deregulation should not be viewed as an exogenous cause of the financial premium but as being part of the continuing political endorsement for the finance sector since the late 1970s.

Two developments are particularly relevant to the finance sector's ability to capture additional income in this account. First, the political involvement of the financial sector has been deepening. Figure 3A presents the relative political involvement of securities and investment firms, measured in terms of campaign donations and lobbying expenses of these firms over those of industrial unions, a main advocate for financial reform and policies that reduce earnings inequality, between 1989 and 2012.¹ The figure shows not only that these firms have consistently outweighed unions in both campaign contributions and lobbying expenses, but also that the gaps have been widening over time. In the late 1980s, these financial firms spent twice as much as unions did in campaign contributions; now they spend almost six times as much. A similar trend is observed for lobbying expenses. The difference between financial firms and industrial unions increased from a factor of 6 in the late 1990s to that of 12 in 2012. The finance sector's expansion in political involvement also outrivals other major industries. In 2012, the securities and investment industry channeled more resources into federal elections than any other sector of the economy, spending 50 percent more than health professionals in the midst of healthcare reform, and 3.5 times as much as the oil and gas industry.

These political activities have real consequences. Fellowes and Wolf (2004) find that business campaign contributions promote favorable regulatory and tax policies. De Figueiredo and Edwards (2007) show that campaign contributions have a robust influence on regulatory outcomes, in the context of telecommunication. Cooper, Gulen, and Ovtchinnikov (2010) show that firm-level political campaign contributions are positively associated with future stock returns between 1979 and 2004. In the finance sector, Igan, Mishra, and Tressel (2011) find that lobbying mortgage lenders were more likely to engage in high-risk lending during 2000 and 2007, which subsequently produced a higher delinquency rate during the financial crisis. They also report that lobbying lenders were more likely to receive funds under the Troubled Asset Relief Program (TARP) and that the size of the bailout was positively associated with cumulated lobbying expenditures.

Besides favorable policies and regulatory practices, Hacker and Pierson (2011) suggest that politically driven failures to update policies and to enforce regulations, respectively, are equally responsible for excessive earnings in the finance sector. A well-known example in this regard is the "carried-interest" provisions in current tax laws, which treat hedge fund managers' fees as capital gains and therefore tax them at a rate of only 15 percent. Freeman (2010) also

Figure 3. The political influence and the bank sector concentration**A** Political influence of securities and investment over industrial unions, 1989–2011**B** Concentration of assets in the banking sector, 1933–2012**Source:** A. OpenSecrets.org, FollowTheMoney.org; B. FDIC, Statistics on Banking.

points out that, long before the subprime mortgage crisis, the Federal Bureau of Investigation detected an abnormal trend in mortgage fraud, which grew exponentially between 1997 and 2007, but the Federal Reserve paid little attention to this warning.

The second development is the increasing concentration of market power among the financial institutions. Figure 3B presents the concentration of assets

in the banking sector. This figure shows that there has been a tremendous consolidation since the 1990s. The three largest bank-holding companies once owned about 10 percent of the total banking assets; now they control over 35 percent, achieved through a relentless acquisition of community-based banks (DePrince 2005). In recent years, more than one-half of commercial banking assets in the United States have been controlled by only 10 holding companies. A similar level of concentration is observed in the mutual fund industry, where the 10 largest companies take about 45 percent of the market share (Ferreira and Ramos 2009).

What is more significant than the horizontal consolidation in the banking sector is the dismantling of the firewalls built to prevent systemic collapses. Many large financial institutions today simultaneously offer services that used to be provided separately by investment banks, commercial banks, savings and loan associations, insurance firms, and public exchanges. An increasing consolidation of the financial sector implies that large incumbent firms hold great power to condition the markets and to collectively demand political intervention when their profitability is threatened. The oligarchical structure also facilitates fraud (Lattman 2013) and anti-trust violations (Eisinger 2012; Silver-Greenberg 2012; Taibbi 2012), and generates frequent conflicts of interest (Bowley 2010; Nocera 2013). Moreover, the absence of threats from challenger firms provides little incentive for incumbent firms to maintain their reputations.

The combination of coordinated political involvement and market consolidation provides the financial sector great ability to extract additional resources, both intentionally and unintentionally, from the rest of the economy. As large financial service firms became cross-industry holding companies, the existing regulatory apparatus, one that still mirrors the fragmentation mandated by the Glass-Steagall Act, became increasingly ineffective in overseeing the interconnection of various financial activities and preventing profit-driven practices that negatively affect consumers of financial products (Tomaskovic-Devey and Lin 2013). Furthermore, because many regulatory agencies, such as the Office of the Comptroller of the Currency (OCC) and the Office of Thrift Supervision (OTS), do not receive governmental funding and instead maintain their operations through regulatory fees paid by financial institutions under their purview, these agencies compete with one another to gain popularity among the financial institutions. Facing declining revenue and staff due to the savings-and-loan crisis of the 1990s, for example, the OTS decided to aggressively remarket itself as a regulator against regulation. This course change attracted various financial institutions, including American International Group and Countrywide, which both set up a savings-and-loan function to switch their regulation to the OTS.

Over the past few years, the combination of political involvement and market consolidation also has allowed the finance sector to guide the public discussion and make effective claims in the formation of recovery policies. The apparent political vacuum created by the financial crisis was immediately filled by the belief that a federal bailout was urgent, while no major conditions were imposed on firms receiving assistance. Within the established networks of influence, this view quickly developed into a seemingly self-evident conviction that preserving

the existing financial system is essential to the recovery of the US economy. Monetary policies that favor Wall Street have been prioritized over fiscal policies that directly benefit Main Street. These developments all indicate that the excessive earnings should remain robust in the aftermath of the financial crisis.

The discussion above yields three distinct predictions with regard to the dynamics of the financial premium. If the premium is a result of the interaction between skill and technological change, we should see the premium smoothly increase in concordance with the earnings potential, an indication commonly used in labor economics for underlying productivity. If the observed financial premium is induced by a demand shock and the US labor market is after all competitive, we should see the financial premium decline in the 2000s, when deregulation slowed down and in the aftermath of the financial crisis, when the demand decreased. If the financial premium is driven mostly by elite workers' power to condition the market through political actions and industry-wide consolidation, we should see the premium disproportionately concentrate at the upper end of the distribution. Furthermore, since the levels of political involvement and consolidation are largely intact during the financial crisis, we should expect the premium to remain stable even when the demand contracts. I conduct the investigation with the guidance of these hypotheses.

Data, Variables, and Methods

Data

I use the Current Population Survey (CPS) March files (King et al. 2010) and inflation-adjusted logged annual earnings to examine the evolution of the financial premium. Monthly data (e.g., Merged Outgoing Rotation Groups) and wages are not preferable, because they do not capture bonuses, a crucial form of compensation for financial and other professional workers. The sample includes full-time (35+ hours) full-year (50+ weeks) employees aged 25–65 that have annual earnings of at least 100 in 2001 dollars. The finance sector is defined as the combination of the banking and securities industries.²

In addition to the repeated cross-sectional data set, I match individuals in the consecutive March files to create a two-year panel data set.³ Individuals are matched using the combination of state, household identifier, household number (which changes when a new family moves into the sampled housing unit), individual line number, gender, and racial identification. The matched observations are then verified by the age difference across time. Following Madrian and Lefgren's (1999) recommendation, I drop both observations from the panel when the change in age is less than -1 or larger than 3 .

Since the primary sampling unit in the CPS is household, individuals who moved away from their original household are not included in the panel. The matching procedure also excludes those who changed their gender or racial identification. To address the potential selection issue, I estimate a series of year-specific logistic regressions that include race, gender, age, employment, level of education, marital status, number of children, and the state where the

respondent resided to predict the likelihood that the same individual would be identified in the subsequent survey. I then divide the sampling weights with the predicted probabilities of entering the panel to moderate the bias. Appendix Table A1 reports the matching rates across years. It should be noted that some consecutive years cannot be matched due to occasional redesigns of the CPS (see Madrian and Lefgren [1999] for technical discussion; Ziliak, Hardy, and Bollinger [2011] for similar matching results).

Both cross-sectional and panel data sets have their own respective strengths. The panel data set has repeated observations for the same individual and thus provides an opportunity to examine whether the financial premium can be attributed solely to unobserved skill differences, a fundamental explanation in the “skill-biased” account. Yet, the survey design of the CPS precludes the linkage of individuals who moved away from their original housing unit. The cross-sectional data, in contrast, are population representative and thus provide more complete coverage of the US labor market. I present the results from both data sets in this article.

While the CPS provides detailed information on earnings and workers’ characteristics, a well-known limitation is that the survey imputes the earnings of the top earners to ensure anonymity. The proportion of earnings that were top-coded slowly increased over time. For women, the number increased from 0.02 percent in 1975 to 0.86 percent in 2007; for men, the number increased from 1.18 percent to 2.59 percent (see Burkhauser and Larrimore [2009] for more discussion on this issue).⁴ Thus, this study is unable to examine the earnings dynamics at the very top (e.g., 1 percent) of the distribution. However, when the scope of analysis is restricted to those at or below the 95th earnings percentile, the main findings of this study are unaffected by top-coding. I will return to this issue when discussing the models.

Variables

The outcome of interest is total pretax wage and salary income in the previous year. Since 1980, the survey specifically prompts the respondents to include overtime pay, tips, bonuses, and commissions. A previous study (Kim and Tamborini 2014) indicates that higher earners tend to underreport their earnings in the survey setting (in contrast to their tax records). However, it is unclear whether those in the finance sector are more likely to do so than those in the non-finance sector. Furthermore, certain types of non-wage compensation (such as carried interest received by hedge fund managers) may not be captured by this item. As a result, the analysis is likely to underestimate the financial premium at the upper end.

Unless otherwise specified, the control variables in the cross-sectional analysis include year; age and its squared term; the interaction between region and metropolitan area status; race; level of education; and the three-way interaction between gender, marital, and parental status.⁵ For the panel models, I include earnings from the previous year to absorb unobserved individual differences.

Table 1 presents the weighted descriptive statistics of the cross-sectional and panel data sets for the years 1980, 1990, 2000, and 2010. This table shows a

Table 1. Descriptive Statistics for the Cross-Sectional and Panel Datasets, 1980–2010

	1980		1990		2000		2010	
	CS	Panel	CS	Panel	CS	Panel	CS	Panel
Inflation-adj earnings	10.634	10.724	10.601	10.705	10.670	10.767	10.669	10.762
(SD)	0.570	0.554	0.627	0.617	0.684	0.681	0.681	0.670
95th	11.492	11.660	11.550	11.724	11.752	11.782	11.775	11.900
90th	11.282	11.415	11.333	11.429	11.464	11.534	11.513	11.578
50th	10.653	10.795	10.639	10.733	10.643	10.733	10.636	10.730
10th	9.960	10.094	9.841	9.983	9.855	9.954	9.879	9.968
5th	9.743	9.886	9.618	9.778	9.632	9.742	9.616	9.783
Finance sector	0.041	0.045	0.043	0.045	0.045	0.046	0.048	0.051
Age	40.561	41.270	39.715	40.416	41.236	42.172	42.978	43.952
(SD)	11.184	10.853	10.211	9.947	10.006	9.619	10.897	10.694
Primary	0.194		0.128		0.105		0.077	
High school	0.424		0.411		0.326		0.291	
Some college	0.188		0.216		0.285		0.283	
College	0.126		0.162		0.204		0.238	
Advanced	0.069		0.084		0.080		0.111	
White	0.834		0.792		0.711		0.678	
Black	0.090		0.098		0.110		0.103	
Hispanic	0.052		0.076		0.122		0.146	

(Continued)

Table 1. continued

	1980		1990		2000		2010	
	CS	Panel	CS	Panel	CS	Panel	CS	Panel
Other	0.023		0.034		0.057		0.073	
Male	0.653	0.687	0.599	0.625	0.587	0.607	0.567	0.592
No child	0.433	0.519	0.476	0.571	0.495	0.577	0.515	0.633
1 Child	0.207	0.199	0.211	0.184	0.199	0.179	0.198	0.157
2 Children	0.218	0.184	0.208	0.169	0.202	0.169	0.187	0.142
3+ Children	0.142	0.097	0.105	0.076	0.104	0.075	0.100	0.068
Married	0.711	0.741	0.647	0.686	0.621	0.669	0.603	0.644
Single	0.125	0.106	0.178	0.154	0.192	0.167	0.221	0.199
Other	0.164	0.153	0.175	0.161	0.187	0.164	0.176	0.157
Metropolitan	0.759		0.821		0.844		0.868	
New England	0.061		0.059		0.052		0.051	
Middle Atlantic	0.170		0.158		0.140		0.139	
East North	0.193		0.181		0.168		0.155	
West North	0.069		0.072		0.072		0.071	
South Atlantic	0.164		0.178		0.191		0.194	
East South Central	0.057		0.056		0.059		0.056	
West South Central	0.102		0.102		0.105		0.116	
Mountain	0.046		0.050		0.061		0.068	
Pacific	0.139		0.145		0.151		0.148	
N	29,533	17,508	31,570	18,797	50,737	18,268	43,785	19,215

clear divergence in inflation-adjusted earnings between the upper end and the lower end of the earnings distribution. While the 95th and 90th percentiles experienced a steady growth over the past two decades, the middle and bottom of the distribution faced either stagnation or a nontrivial decline. It also shows that the relative size of the finance sector has been increasing over time, from 4.1 percent (4.5 percent in the panel data) in 1980 to 4.8 percent (5.1 percent in the panel data) of the total full-time, full-year workforce in 2010. There are also some differences between the cross-sectional and panel data sets. Workers in the panel data, on average, have higher earnings but lower dispersion, are older, and are more likely to be male and married. These differences are due to the exclusion of movers from the panel.

Re-Centered Influence Function Regression

I use re-centered influence function (RIF) regressions (Firpo, Fortin, and Lemieux 2009; Fortin, Lemieux, and Firpo 2011) to detect how the financial premium varies across the earnings quantiles and to estimate the impacts of the financial premium on growing inequality. The logic of this method is based on the statistical concept of influence function, the relation between a single data point and the statistics of interest, such as quantile, variance, or Gini coefficient. By re-centering the influence function with statistics of interest and regressing each observation's influence on the explanatory variables, one can estimate how these variables jointly shape the unconditional statistics of interest. In this light, the standard OLS model could be viewed as a re-centered influence function regression, where the statistics of interest is the mean and the influence function is $Y - \mu$, and the re-centered influence function is simply $Y - \mu + \mu$. In the case of quantiles, the re-centered influence function for the τ th quantile is

$$RIF(y; q_\tau) = \frac{\tau - 1\{y \leq q_\tau\}}{f_Y(q_\tau)} + q_\tau \quad (1)$$

where y denotes the observed outcome; $1\{y \leq q_\tau\}$ is an indicator function, which equals 1 when y is equal or smaller than q_τ and equals 0 otherwise; and $f_Y(q_\tau)$ is the probability density of Y at q_τ , which is approximated through a kernel function in the analysis.

Analytically, unconditional quantile regression provides significant advantages over more common estimation techniques. While standard regression compares the mean differences, unconditional quantile regression allows researchers to estimate quantile-specific effects. This is a useful feature, especially as an increasing number of studies are showing that the effects of explanatory variables are rarely uniform throughout the distribution of outcomes. The best-known example in the research on inequality is the effect of union status on wages (Freeman 1980; Western and Rosenfeld 2011). Union workers earn more than non-union workers, on average, but the positive effect diminishes for workers with higher wage potential and turns negative for workers at the top (see figure 6 in Lemieux [2008]). Precisely because of this mean-reverting nature, union density tends to

reduce the dispersion of the wage distribution (Card 1996; Lin and Tomaskovic-Devey 2013; Western and Rosenfeld 2011). Similarly, the effect of the minimum wage is salient at the bottom of the wage distribution but becomes negligible above the bottom (Autor, Manning, and Smith 2010; Neumark, Schweitzer, and Wascher 2004).

It should be emphasized that the quantile estimates obtained by the re-centered influence function regression are conceptually different from those of the standard quantile regression (Hao and Naiman 2007; Koenker and Bassett 1978; Koenker 2006). The standard quantile regression obtains the coefficients through the asymmetrically weighted minimization of the residuals. Thus, the associations are estimated across the conditional or residual distribution. By contrast, the RIF regression approximates the effects at the unconditional quantiles, and the coefficients are thus more readily interpretable. Previous applications of the standard quantile regression (e.g., Budig and Hodges 2010; Penner 2008) overlook this distinction and interpret the standard quantile estimates as if they were across the unconditional distribution of the outcome.

For cross-sectional estimates, I detect the size of the financial premium with

$$RIF(y_{t,i}; q_{\tau,t}) = \alpha_{\tau,t} + \beta_{1,\tau,t} F_{t,i} + \sum_k^K \beta_{k,\tau,t} X_{k,t,i} + \varepsilon_{t,i} \tag{2}$$

where t denotes time period, F indicates whether worker i is in the finance sector, and X denotes a series of covariates. For the panel estimates, I specify the models with an additional lagged term for annual earnings:

$$RIF(y_{t,i}; q_{\tau,t}) = \alpha_{\tau,t} + \beta_{0,\tau,t} y_{t-1,i} + \beta_{1,\tau,t} F_{t,i} + \sum_k^K \beta_{k,\tau,t} X_{k,t,i} + \varepsilon_{t,i} \tag{3}$$

where $y_{t-1,i}$ represents the logged earnings from the previous year, which absorbs unobserved individual differences that are associated with earnings and reduces the omitted-variable bias. The lagged dependent variable model is preferred over the common fixed-effect approach in this case because adding fixed-effect terms to the models is likely to create the incidental parameters problem and produce biased estimates. As such, I estimate specific effects on quantiles, taking into account individuals' prior earnings, instead of the financial premium for individuals who switched in or out of the financial sector.

In contrast to the models that regress the observed values on the explanatory variables, the unconditional quantile regression transforms the observed values into quasi binaries that are either above or below a given unconditional quantile (see Equation (1)). Since all the top-coded observations (fewer than 3 percent of the sample) are certainly greater than the quantiles of interest, the top-coding procedure will have no effect on the estimates.

To assess the impacts of the financial premium on the distribution of earnings, the earnings at the τ^{th} quantile at time t can be written as

$$E[RIF(y; q_{\tau, t})] = \alpha_{\tau, t} + \beta_{1, \tau, t} \bar{F}_t + \sum_k^K \beta_{k, \tau, t} \bar{X}_{k, t} \quad (4)$$

The difference in the τ th quantile between t_1 and t_2 then can be decomposed as the sum of two components:

$$\begin{aligned} E[RIF(y; q_{\tau, t_2})] - E[RIF(y; q_{\tau, t_1})] &= (\beta_{1, \tau, t_2} \bar{F}_{t_2} - \beta_{1, \tau, t_1} \bar{F}_{t_1}) \\ &+ \left(\sum_k^K \beta_{k, \tau, t_2} \bar{X}_{k, t_2} - \sum_k^K \beta_{k, \tau, t_1} \bar{X}_{k, t_1} \right) \end{aligned} \quad (5)$$

where $\beta_{1, \tau, t_2} \bar{F}_{t_2} - \beta_{1, \tau, t_1} \bar{F}_{t_1}$ represents the joint contribution of the employment share and the earnings premium in the finance sector, and the effect of the financial premium can be isolated with the Oaxaca-type decomposition as⁶

$$\bar{F}_{t_1} (\beta_{1, \tau, t_2} - \beta_{1, \tau, t_1}) \quad (6)$$

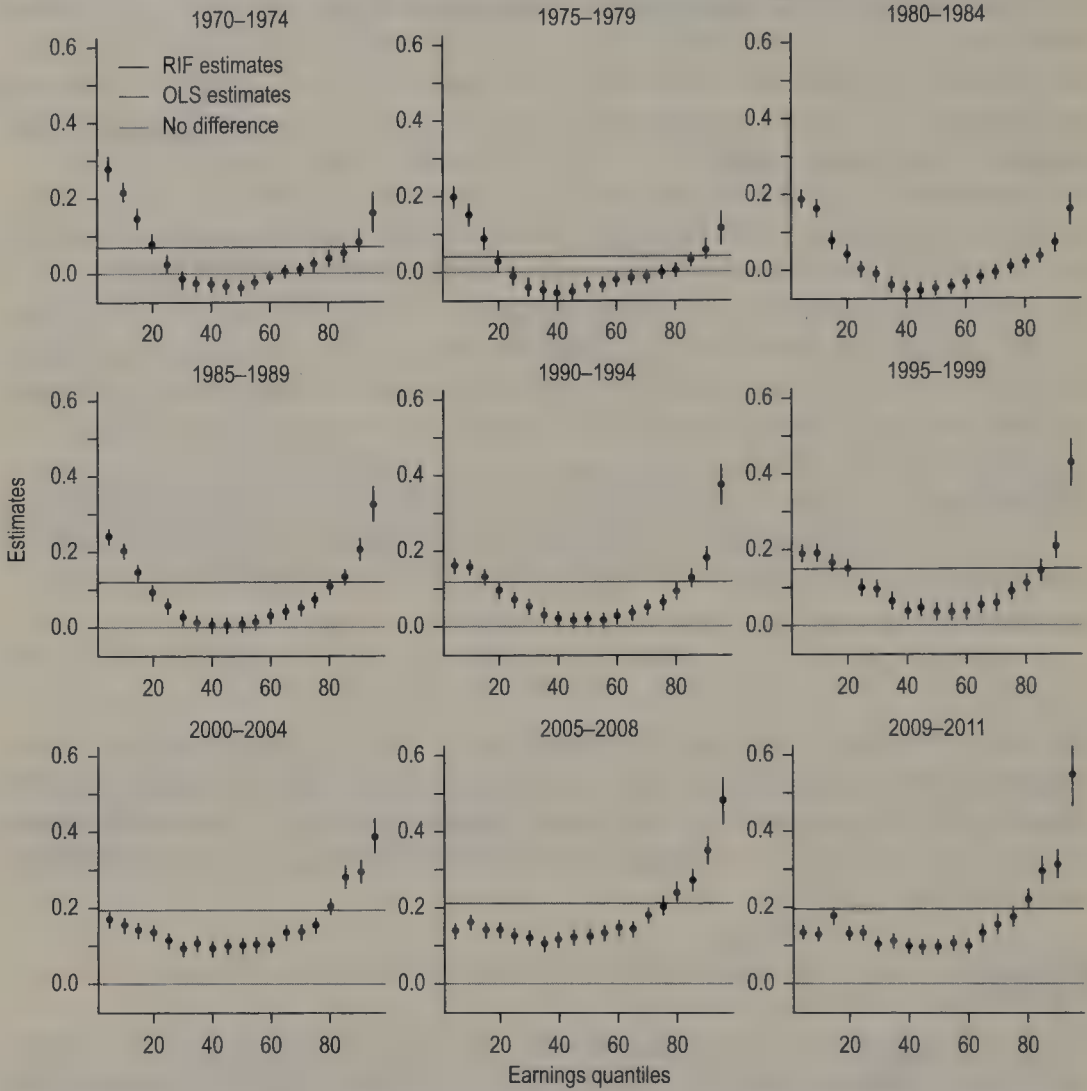
The full investigation requires the estimation of 323 regression models (9 time periods and 19 quantiles for the cross-sectional estimates; 8 periods and 19 quantiles for the panel estimates). In the following section, I present only the estimates for the financial premium. The estimates for the control variables are available upon request.

Results

Figure 4 presents the period-specific estimates of the financial premium across the unconditional earnings quantiles, where the horizontal lines represent standard OLS estimates. The figure shows that the financial premium was present in the 1970s but mostly concentrated at the bottom of the earnings distribution. Low-paid financial workers tended to earn between 20 and 30 percent more than their non-financial counterparts. This result is unexpected, considering the low-wage workers in the financial sector, or service sector in general, were rarely unionized. A potential explanation is that the finance sector is much more vulnerable to internal theft and fraud than other industries. Paying low-wage workers more could attract more honest workers, create positive social norms, and increase the cost of losing jobs (Chen and Sandino 2012). It should be noted that the premium for these low-paid financial workers shrank from 0.3 to 0.2 during this period, which may be a result of the takeoff of Automatic Teller Machines (ATMs) throughout the 1970s.

The premium declines to below zero as the earnings approach the 40th percentile but increase incrementally above the 40th percentile. In the early 1970s, financial workers at the top of the distribution (95th percentile) earned about 15 percent more than the non-financial baseline. Overall, the average premium

Figure 4. Financial premium across the earnings quantiles, cross-sectional estimates



Source: IPUMS-March CPS. The 95 percent confidence intervals are calculated with robust standard errors.

observed in the 1970s (see figure 1) is largely due to the bonuses paid to low-wage workers. As such, the reverse J-shaped compensation structure of the finance sector actually reduced societal inequality in the 1970s.

The pattern started to change in the 1980s, during which time the financial regulations set in place to prevent another Great Depression were gradually loosened and the average financial premium achieved significant growth. While the financial workers at the bottom still had similar advantages to those they had in the 1970s, there was a broad-based increase in the earnings differential across the rest of the distribution. By the second half of the 1980s, financial workers in the middle earned either similar or above-market compensation. The growth is disproportional and most dramatic at the upper end, which tripled its premium to above 30 percent and transformed the reverse J-shape in the 1970s to a U-shaped premium structure.

A similar trend unfolded in the 1990s, the dawn of the rapid consolidation in the banking sector (see figure 3B). In this period, however, we observe a strong departure of elite workers from the rest of the distribution. While the premium gap between the 95th and 90th percentiles had been proportional to the subsequent gaps in the previous two decades, it far exceeded any extrapolation in the late 1990s. Financial workers at the 95th percentile now earned 40 percent more than their nonfinancial counterparts, a premium four times greater than the average OLS estimate, while those at the 90th percentile had only one-half of that advantage. This clear discontinuity contradicts skill-based explanations, if one considers unobserved skill difference as largely continuous. Yet, it is consistent with the winner-take-all account, that elite workers have largely harvested the growth in overall earnings.

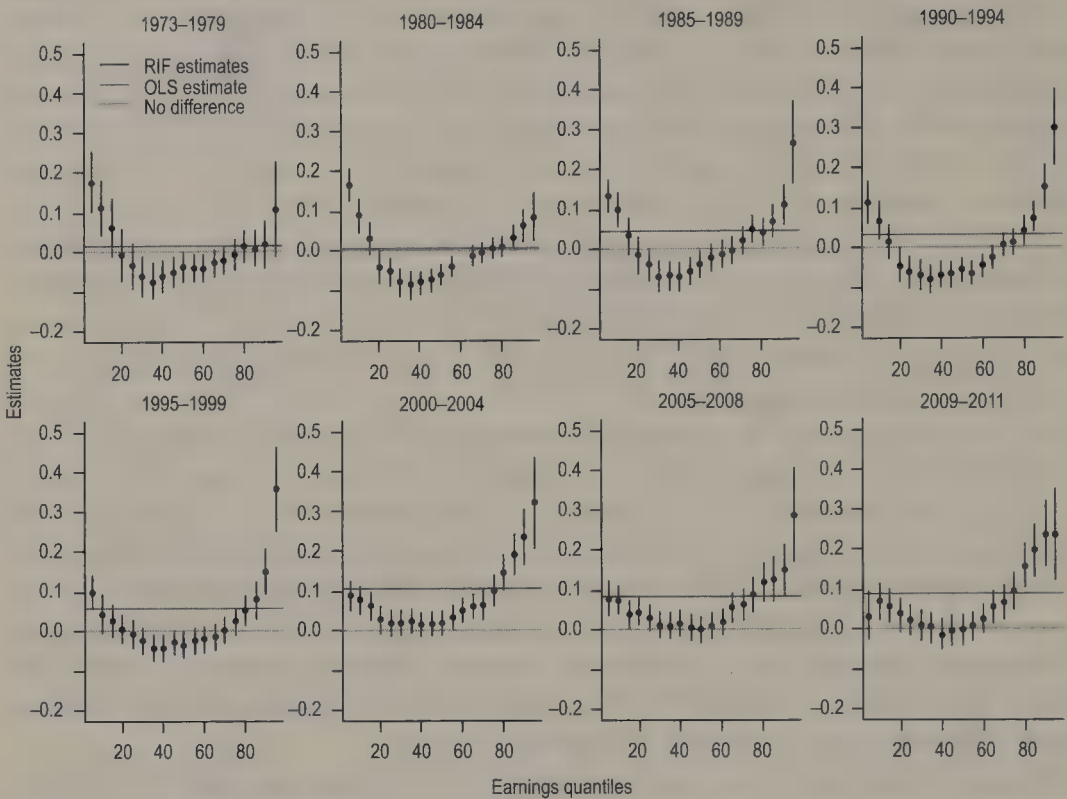
In the 2000s, we see a continued increase in the financial premium for the middle of the distribution, which transforms the U-shape in the 1980s into a new J-shaped structure. The advantage at the bottom declined slowly in this period, and thus low-paid financial workers became less distinguishable from other low-paid workers. The premiums for the 90th and 85th percentiles caught up to the top in the early 2000s, but the premium gaps widened again before the most recent financial crisis. It should also be noted that, while there was little change in the average financial premium during the 2000s, bonuses for workers at the 95th percentile increased further to 50 percent.

Finally, I examine the earnings dynamic in the post-crisis period (2009–2011). The result shows that, for the financial workers under the 90th percentile, the financial crisis had little impact on their relative earnings. What is most striking is that the relative compensation at the very top seems to have been completely unaffected by the financial crisis, while a contraction in demand was occurring during that period. The large discrepancy between the 95th and 90th percentiles again supports the argument that the financial premium is less driven by the gravity of supply and demand than control over the markets.

Figure 5 presents the results from the panel estimates. As expected, adding the lagged term of annual earnings yields a more conservative picture. The overall pattern, however, is similar. The financial premium in the 1970s was driven mostly by the relatively high compensation at the bottom of the distribution, while the recent financial premium is driven by the excessive returns to the elite workers. The estimates also suggest that workers in the middle of the distribution might receive no premium or even a penalty for working in the finance sector, while there is significant payoff for elite workers, even when accounting for unobserved individual differences. In particular, from 1985 to 2008, financial workers at the 95th percentile consistently gained a significantly greater premium than those at the 90th, a discontinuity that is difficult to reconcile with the skill-based account.

Figure 6 decomposes the finance sector's contribution to widening earnings inequality, using cross-sectional estimates. Earnings in the finance sector could shape the labor-market-wide earnings distribution through three mechanisms: changes in the workforce characteristics of financial workers, changes in the finance sector's employment share, and changes in the financial premium.

Figure 5. Financial premium across the earnings quantiles, panel estimates



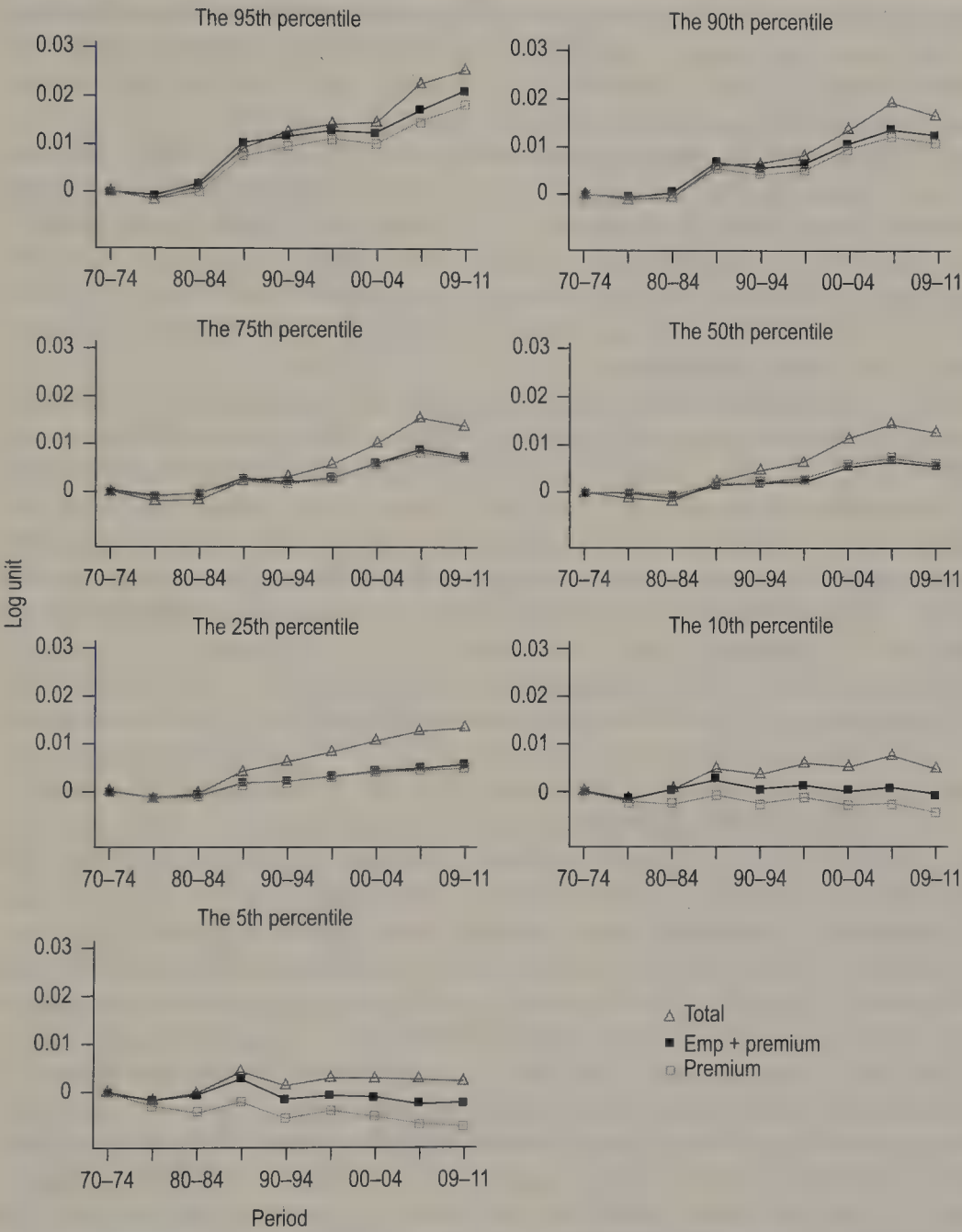
Source: CPS Utilities-March Files. The 95 percent confidence intervals are calculated with robust standard errors.

The figure shows that the total contribution of finance to the earnings quantiles has been increasing since the 1980s, especially at the top end of the distribution. Estimates show that more than 11 percent (0.0258 log units) of the total growth (0.2215 log units) in earnings at the 95th percentile between 1970 and 2011 can be attributed to shifts associated with the finance sector, 70 percent of which is solely driven by the rise of the financial premium. By contrast, though the finance sector as a whole also increases earnings at the bottom, the magnitude is relatively small. Shifts in the financial premium have a net negative impact at the 5th and 10th percentiles due to the declining rent for low-wage financial workers (see figure 4).

Discussion

This study revisits the evolution of financial premium between 1970 and 2011. By examining the financial premium across earnings quantiles with both cross-sectional and panel data sets, I report a new and more complete picture of the earnings dynamics associated with the rise of finance. The result shows that above-market compensation was present in the finance sector in the 1970s, but it was heavily concentrated at the bottom end of the earnings distribution. As a result, the compensation structure of the finance sector actually reduced overall inequality prior to the 1980s. The rise of financial premium observed since the

Figure 6. Contribution of the finance sector on the concentration of earnings



Source: IPUMS-March CPS, 1970–1974, period is used as the baseline. I calculate the total contribution of the finance sector by estimating a series of RIF regressions with only the finance indicator in the models. The total contribution from 1970 to 1974 to a given period is calculated as $\beta_{1,\tau,t2} F_{t2} - \beta_{1,\tau,t1} F_{t1}$. The Employment Size + Premium series is calculated in a similar way, but this time with the control variables such as education and demographic characteristics in the RIF models. The contribution of the premium is isolated using Equation (6).

1980s, in contrast, has been driven largely by excessive compensation for workers at the upper end of the distribution. The cross-sectional estimates show that in recent years, the premium for the 95th percentile is twice as much as that for the 90th percentile and five times greater than that for the median. The analysis also suggests that the financial premium, particularly of those at the top, was virtually unaffected by the financial meltdown and the subsequent credit contraction. As a result, the finance sector has been increasingly contributing to the concentration of earnings at the top. The decomposition analysis suggests that more than 11 percent of the earnings growth at the 95th percentile since the 1970s can be attributed to shifts associated with the finance sector, 70 percent of which is driven by an increase in the earnings premium rather than changes in the finance sector's size and workforce characteristics.

The findings contradict the account that the financial premium is driven primarily by unobserved skill difference. First, taking unobserved heterogeneity into account does not eliminate the premium for elite financial workers. Even when taking the earnings from the previous year into account, elite financial workers still out-earn their counterparts in the non-finance sector by a margin of 25 to 35 percent. Second, a skill-based explanation does not explain why there is a discontinuity in the unobserved skill difference at the upper end of the distribution, instead of increasing incrementally with the earnings potential. The discontinuity is most evident since the late 1980s in both the cross-sectional and panel estimates. Repeatedly, we observe that financial workers at the 95th percentile receive a premium that is two to three times greater than that of financial workers at the 90th percentile. Third, Philippon and Reshef (2012) argue that the financial premium is partially due to the demand shock resulting from a series of deregulations. The estimates here indicate, however, that the premium persisted in the 2000s, when the deregulation movement slowed down. This account also does not reconcile with the finding that the premium for elite workers remained robust in the post-crisis period, during which there is a contraction in demand. In contrast, these results are more consistent with the account that the financial premium was largely created and has been sustained by a combination of political involvement and consolidated market power.

By contrast, these findings are consistent with the institutional account of the financial premium. As levels of political involvement and market consolidation started to increase in the late 1980s, the earnings premium for elite financial workers began to soar. With the regulatory agencies following industrial preferences, the financial sector can afford to pursue high-reward activities without considering the risks involved. A consolidation of the financial sector also implies that the incumbent firms hold great power to condition the market and to collectively demand political inaction when there are signs of abnormality, and stage an intervention when their profitability is threatened. Market consolidation also effectively creates opportunities for social closures that facilitate fraud and anti-trust violation, and generates conflict of interests. In sum, the combination of coordinated political involvement and market consolidation provides the financial sector great ability to extract resources from the rest of the

economy prior to the financial crisis, and to promote capital-friendly recovery policies that sustain their interest.

While the evidence presented in this article does not indicate skill upgrade as a primary cause of the financial premium, educational credential is likely to serve as a barrier of entry that sustains and legitimizes the earnings differential at the upper end of the distribution. By exclusively recruiting graduates from elite schools (Binder 2014) and selecting only workers with the highest social similarity with existing employees (Crotty 2009; Rivera 2011, 2012), the incumbents effectively suppressed the supply of skilled labor into finance.

Although this article focuses on the financial premium, the mismatch between productivity and compensation is unlikely to be unique to the financial industry. A labor-market version of the efficient-market hypothesis, which presumes that compensation is tightly coupled with productivity, tends to ignore that resources are extracted and allocated through a variety of collective mechanisms. Between organizations, resources are frequently transferred due to market power, subsidies, threats, scams, and charity. Within organizations, resources are regularly distributed on the basis of organizational inertia, loyalty, friendship, and bureaucratic consistency. Economic agents are often rewarded or punished in line with their ascribed and subscribed memberships (Tilly 1998; Tomaskovic-Devey 2014). The avoidance of competition through forms of social closure, such as licensure (Weeden 2002) and categorical discrimination (Roscigno, Mong et al. 2007; Roscigno, Garcia, and Bobbitt-Zeher 2007), is not an outlier but a common practice.

Due to the limitations of the CPS, this study is unable to examine the financial premium at the very top, such as the 1 or 0.1 percent of the labor market. The findings suggest, however, that these workers are likely to receive a net premium that is exponentially greater than those received at the 95th percentile of the earnings distribution. If this is the case, the growing financial premium since the 1980s may be a critical contributing factor to the concentration of income at the very top of the US labor market.

Although the analysis above shows that the evolution of the financial premium directly widens the earnings distribution, the ascendance of the finance sector may also contribute to the earnings inequality indirectly. As financial professionals increasingly become the ideal candidates for executive positions (Fligstein 1993; Zorn 2004), they also bring in managerial strategies that focus on return to investment and market-driven flexibility. The shareholder-value model of corporate governance promoted by financial professionals also transfers resources from the firm to the top executives and the investors (Shin 2012, 2014). These developments are likely to further widen the overall earnings distribution in the labor market.

Finally, this article demonstrates that distribution analysis has the potential to provide significant insights, particularly in an era of rising inequality (see Killewald and Bearak [2014]; Western and Rosenfeld [2011] for examples). The vast majority of the quantitative literature in sociology focuses on the average effect, while in reality the relationships between explanatory and outcome variables tend to vary or even reverse across the distribution. Being a racial minority,

for example, might have vastly different implications on earnings prospects, depending on the individual's labor market location. A shift of interest from the single average effect to distribution-specific effects is likely to yield more nuanced understandings of the social world.

Notes

1. The aggregate campaign contribution and lobbying expense are estimated by the Sunlight Foundation. Campaign contribution is calculated based on itemized contributions reported to the Federal Election Commission and state agencies. The total amount includes contributions from the employees of the organizations, their family members, and their political action committees. Contributions under \$200 are not included in the statistics. The lobbying expense is calculated based on lobbying activity reported to the Senate Office of Public Records. See Hacker and Pierson (2011) and Hillman, Keim, and Schuler (2004) for more discussion on the role of political expenditure.
2. Technically, the finance sector in my analysis includes 707–709 in the 1971–1982 SIC, 700–710 in the 1983–2002 SIC, and 6870–6970 in the 2003–2012 North American Industry Classification (NAICS). It should be noted that in the NAICS, some workers in the banking industry are categorized under Management of Companies and Enterprises, and therefore are not included as financial workers in the analysis. The switch in classification is likely to produce a downward bias, because these workers tend to have higher compensation. Descriptive statistics in table 1 indicate that the new classification has no apparent effect on the size of the finance sector, and thus the bias is likely to be small.
3. CPS households are in the survey for four consecutive months, out for eight months, and then return for another four months before leaving the sample permanently. The same individual is expected to be interviewed twice in the March survey.
4. A further complication is that the Census Bureau changed its top-coding procedure in 1996. Instead of assigning one single value to all top-coded earnings, the Census Bureau constructed 12 socioeconomic groups (based on race, gender, and whether the respondent works full-time, full-year), set group-specific thresholds, and started to assign group-specific means (i.e., the average of all top-coded earnings) to each groups. To make the top-coding procedure consistent over time, I re-impute the top-coded earnings with the top-code cell means from the restricted CPS data (Larrimore et al. 2008). As discussed here and in the following section, the change in top-coding only has an effect on the estimates of average premium (e.g., figure 1) but not the estimates of quantile-specific premium.
5. The region and metropolitan area interaction accounts for the concentration of financial firms in high-wage areas. The gender, marital, and parental-status control accounts for the different effects of parental status for men and women (Budig and England 2001; Correll, Benard, and Paik 2007; Killewald 2013). I do not include work hours in the model, because workers with longer workweeks tend to report even more weekly hours. See Lin (2012) for discussion. I do not include occupation because occupational classification may be endogenous. Indeed, many holding sales positions in the finance sector are recategorized as professionals when their educational credential and earnings increase. Overall, the inclusion of these variables does not change the substantive findings reported in this study. Alternative estimates are available upon request.
6. The assumption here is that the size of the financial premium is independent from the proportion of workers that are in the finance sector.

Appendix

Table A1. Matching Rate and Number of Observations by First Interview Year

Year	Original obs	Matching rate	Matched obs	Filtered obs	% of matched obs
1973	68448	47.82%	32729	7578	23.15%
1974	65255	73.18%	47752	11010	23.06%
1979	77423	68.27%	52855	12566	23.77%
1980	91102	70.56%	64281	15580	24.24%
1981	89568	63.41%	56792	13882	24.44%
1982	81618	69.93%	57075	13716	24.03%
1983	81142	68.39%	55491	13444	24.23%
1984	80381	66.21%	53224	13772	25.88%
1986	78247	65.78%	51472	14458	28.09%
1987	77772	67.44%	52451	15378	29.32%
1988	78119	62.68%	48962	15054	30.75%
1989	72595	68.45%	49688	15652	31.50%
1990	79317	67.49%	53533	16526	30.87%
1991	78464	67.19%	52718	16658	31.60%
1992	77171	67.67%	52221	16438	31.48%
1993	78155	50.78%	39684	12282	30.95%
1994	69236	51.00%	35312	11318	32.05%
1996	65553	69.14%	45326	15546	34.30%
1997	66394	68.51%	45484	15738	34.60%
1998	66171	68.72%	45474	16148	35.51%
1999	66686	68.87%	45929	16472	35.86%
2000	59671	74.85%	44662	16412	36.75%
2001	64052	65.52%	41969	15666	37.33%
2002	107171	47.55%	50963	18276	35.86%
2003	107029	48.76%	52188	18658	35.75%
2004	104555	43.37%	45346	15974	35.23%
2005	103397	46.42%	48001	16720	34.83%
2006	103741	46.33%	48060	16752	34.86%
2007	102808	47.88%	49223	17376	35.30%
2008	101536	48.38%	49119	17186	34.99%
2009	103156	48.97%	50517	16306	32.28%
2010	103777	47.80%	49603	15574	31.40%
2011	101493	48.75%	49473	15358	31.04%

About the Author

Ken-Hou Lin is Assistant Professor of Sociology and Faculty Research Associate of the Population Research Center at the University of Texas at Austin. His research interests include inequality, finance, organization, race, and quantitative method. His work has appeared in *American Journal of Sociology*, *American Sociological Review*, *North Carolina Banking Institute Journal*, and *Social Science Research*.

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Learning from Performance: Banks, Collateralized Debt Obligations, and the Credit Crisis

Kim Pernell-Gallagher, *Harvard University*

This article investigates how firms in competitive markets use external examples to assess the value of novel practices, focusing on the substantively important case of collateralized debt obligation (CDO) underwriting among US investment and commercial banks, 1996–2007. Diffusion researchers have struggled to adjudicate between competing mechanisms of social contagion, including imitation and learning. I use event-history methods to examine how banks responded to the activities and results of other CDO underwriters. I show that banks learned *superstitiously* from the share price performance of other CDO underwriters; as the popularity of CDO underwriting increased, banks became even more attentive to confirmatory evidence on this dimension. These findings suggest important refinements to theories of social contagion, especially neoinstitutional theory. By focusing on ordinary organizational processes in an extraordinary context, I uncover an alternative explanation for the rise of complex securitization, with implications for current understandings of the credit crisis.

The global financial crisis of 2007–2008 introduced the term “collateralized debt obligation” (CDO) into the popular lexicon. US commercial and investment banks became heavily exposed to credit risk through underwriting these complex financial products, and this practice was directly responsible for multi-billion-dollar write-downs at many financial institutions (Risk 2008; FCIC 2011). The rise of CDO underwriting had major deleterious consequences, yet few accounts of the credit crisis have investigated the factors that drove banks to embrace this practice. Perhaps the reasons seem obvious: banks underwrote CDOs because they are competitive, profit-minded organizations, and they anticipated that this activity would bring them value. But explanations of this kind raise more questions than they answer. How did banks come to determine that CDO underwriting was a valuable practice? What kind of information captured their attention?

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Answering these questions requires a better understanding of the organizational processes that drive firms to embrace innovations. Sociologists have offered the important insight that innovation diffusion may be driven by social contagion—another way of saying that the behavior of a focal actor may be shaped by exposure to the behaviors of other actors. Social contagion is a central focus in the voluminous organizational diffusion literature, yet we know surprisingly little about the mechanisms that drive this process (Dobbin, Simmons, and Garrett 2007; Schneiberg and Clemens 2006; Strang 2011). I posit that scholars of social contagion have yet to sufficiently address a fundamental question: Do firms respond to the *results* or the *prevalence* of other innovation users?

This empirical question divides two theoretical perspectives that highlight different causal mechanisms of social influence. A key tenet of neoinstitutional theory is that the social acceptance of a practice influences perceptions of its value: firms are expected to embrace practices that are popular with others (DiMaggio and Powell 1983; Tolbert and Zucker 1983). Vicarious learning theory focuses on the role of performance feedback: firms are expected to embrace practices that prove successful for others (Cyert and March 1963; Haunschild and Miner 1997; Ingram 2002). These theoretical accounts are not mutually exclusive, but their different emphases have translated into different empirical practices. Neoinstitutional research has generally used the cumulative prevalence of adoption, a measure of social acceptance, to model social contagion, while vicarious learning research has focused on the performance outcomes of prior adopters. Vicarious learning theorists have criticized the exclusive focus on prevalence in neoinstitutional research, arguing that the implicit assumption that “managers pay close attention to what others do, while lacking interest in what happens when they do it,” is unrealistic when managers are evaluated on performance (Strang and Macy 2001, 153). But neoinstitutionalists have offered the equally convincing rejoinder that “what happens when [other firms] do it” is often unobservable, since firms rarely share private performance information with competitors (Greve 2011, 953; Abrahamson and Rosenkopf 1993).

This article introduces a novel mechanism of social contagion that takes both considerations into account. While it is true that firms in competitive markets rarely observe the precise performance effects of innovation use elsewhere, I do not expect them to simply emulate popular activities. Instead, I argue that firms learn *superstitiously* from the observable experiences of other innovation users, responding to general impressions of “how well other users are doing” on broad metrics like profitability or share price. “Superstitious” learning occurs when actors mistakenly attribute outcomes to recent actions that may not have caused the outcome—in other words, firms may embrace an activity because it co-occurred with favorable performance, regardless of whether it actually contributed to performance (Levitt and March 1988, 326). I propose that firms respond to loosely linked performance feedback from other innovation users. Furthermore, they become increasingly attentive to confirmatory evidence on this dimension as the innovation becomes more popular. These propositions suggest a new way to understand how social pressures drive the rise and spread of innovations: firms may not blindly mimic popular innovations, but institutionalization shapes the kind of evidence that catches their eye.

These theoretical propositions have important substantive implications. Most popular and scholarly accounts of the credit crisis have focused on the technological, economic, and regulatory changes that made complex securitization an increasingly attractive profit opportunity (Carruthers 2010; Fligstein and Goldstein 2010; MacKenzie 2011), but I suggest that the way banks came to seize this opportunity provides an equally important, and unexplored, part of the story. In many respects, the CDO was just another innovation that diffused through ordinary organizational processes. By focusing on ordinary organizational processes in an extraordinary context, I uncover an overlooked explanation for the rise of complex securitization, with implications for current understandings of the crisis.

The primary goal of this article is to better explain how firms use external examples to assess the value of novel practices. To this end, I use event-history models of the CDO underwriting history of 268 publicly traded US commercial and investment banks, 1996–2007, to examine how banks responded to the observed results and activities of other CDO underwriters, exploring how these responses changed as the practice became more common in the industry.

The Rise and Fall of CDO Underwriting

“Collateralized debt obligations” (CDOs) are complex securities that derive value from the cash flow associated with a portfolio of securitized assets. CDOs differ from other asset-backed securities (ABS) like mortgage-backed securities in the type of assets included and the structure of credit risk. CDO portfolios generally combine a variety of assets (e.g., bonds, mortgages, other ABS, swaps), and the associated credit risk is subdivided into different risk classes or “tranches.” If cash flow is insufficient to repay all investors, lower tranches bear losses first.

CDO underwriting could be quite lucrative for banks. Underwriting a securities issuance involves purchasing the issuance at a set price and placing it with investors. In exchange for structuring the deal and assuming placement risk, underwriters earn fees equivalent to the difference between the purchase and sale price. CDO underwriters could receive fees as high as 2 percent—five times that for underwriting investment-grade bonds (Cresci 2005). However, exposure to credit and placement risk also means that banks could lose money on CDO underwriting. If the underwriter misjudges investor interest, or if CDO value declines between purchase and sale, the underwriter must take the loss. Additionally, underwriters often retain hard-to-sell tranches to increase investor confidence: when CDOs performed poorly, this practice magnified bank losses (FCIC 2011).

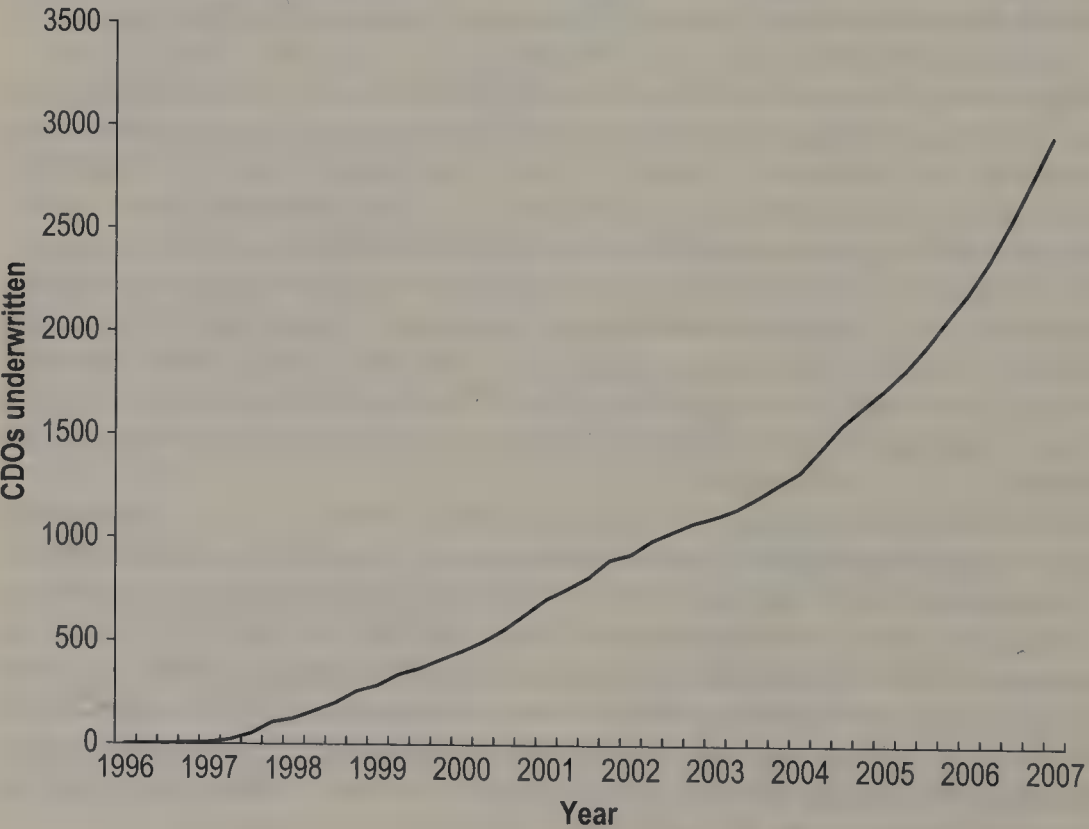
CDOs were not legitimate financial products when they first emerged, but they eventually achieved widespread social acceptance. Investment bank Drexel Burnham Lambert issued the first CDO in 1987, in an attempt to pull wary investors into the declining junk bond market. The initial demand for CDOs collapsed alongside this market in 1989, but reemerged in 1996 following the introduction of a simpler class of CDOs backed by loans or corporate bonds (Tavakoli 2003, 6). These products remained on the fringes of financial markets well into 1997 (Elstein 1997), but by 1999, CDOs had clearly become an accepted part of the financial landscape. By the early 2000s, CDO underwriting was considered a

necessity for major market players: “[d]ealers are realizing that to be a player in the capital markets, you need a CDO business” (Graubard 2001).

In 2001, a decline in the value of corporate debt led to an unexpectedly high number of CDO downgrades, which demonstrated that these financial instruments had risks as well as benefits (Chaffin and Silverman 2001). The CDO market rebounded after this brief legitimacy crisis and went on to experience dramatic growth between 2003 and 2007. By 2006, it was clear that CDOs had achieved widespread social acceptance: they were acknowledged as “one of the most important new financial innovations of the past decade” (Longstaff and Rajan 2008). New issuance surpassed \$312 billion, and CDOs constituted the second-largest ABS sector in the United States (Thompson et al. 2007). Figure 1 reports the cumulative total of CDOs underwritten by publicly traded banks, 1996–2007.

The case of CDO underwriting among publicly traded US banks provides an ideal setting to investigate superstitious vicarious learning. Bank *involvement* with this practice was visible and salient: CDO underwriting is a major undertaking, and it attracted attention from market participants. However, it is unlikely that market participants observed the *revealed value* of CDO underwriting at other banks. The vast majority of CDOs were structured as private placements and underwriters of privately placed securities generally do not disclose deal-specific information, including information on fees (FDIC 2007). Lacking this information, it was difficult for banks to precisely ascertain how much money

Figure 1. Cumulative density of CDOs underwritten by all US publicly traded banks, 1996–2007



other underwriters were making. Potential underwriters could not obtain information on revenue from CDO underwriting from subscription financial data services (e.g., Bloomberg) because these services only report disclosed fees. Additionally, banks did not itemize CDO-related revenue in regulatory reports. Banks likely obtained some information on CDO-related revenue through informal networks or employee turnover, but this kind of information is neither comprehensive nor wholly reliable. Without precise performance information, opportunities for “true” vicarious learning are rare. This is not unique to CDO underwriting; in competitive markets more generally, where proprietary information is closely guarded, firms often lack direct access to the revealed value of innovations adopted by other firms (Abrahamson and Rosenkopf 1993; Greve 2011, 953).

Theories of Social Contagion

The drivers of social contagion have been long-standing topics of debate in the sociological diffusion literature. Early debates focused on the motivations for interorganizational imitation: accounts that emphasized social motives (e.g., the pursuit of legitimacy) opposed accounts that emphasized purely economic motives (e.g., the pursuit of technical efficiency). Neoinstitutional theory is the perspective most closely associated with the “social motives” tradition. Early works in neoinstitutional theory framed institutional pressure as distinct from technical pressure or rational decision-making, and argued that the pursuit of institutional legitimacy represented an additional (and overlooked) driver of social contagion (Meyer and Rowan 1977; DiMaggio and Powell 1983). Organizations were predicted to mimic institutionalized practices, those perceived as “appropriate and necessary component[s] of efficient, rational organizations,” in addition to those that increased technical efficiency (Meyer and Rowan 1977; Tolbert and Zucker 1983, 26).

Empirical studies that tested these predictions tended to use a practice’s *density* or cumulative prevalence in the organizational field as an indicator of its relative institutionalization and social acceptance (Fligstein 1985; Tolbert and Zucker 1983; cf. Mizruchi and Fein 1999). If adoption likelihood increased with density, institutional effects were inferred (Burns and Wholey 1993; Fligstein 1985; Haveman 1993). As neoinstitutional accounts expanded to explain corporate behavior in competitive markets, this empirical strategy attracted critical attention. Rationalist diffusion scholars argued that the finding of density dependence, often framed as a response to institutional pressures, was just as easily explained by competitive pressures: efficiency-seeking firms imitate prior adopters because these adopters have more accurate information about the innovation’s true technical value (Banerjee 1992; Bikhchandani, Hirshleifer, and Welch 1992; Rogers 1983).

Over the past two decades, there has been considerable rapprochement between neoinstitutionalist and rationalist accounts. Most contemporary diffusion scholars do not see economic and social considerations as mutually exclusive: “wanting to look good does not also preclude wanting to do better” (Kennedy and Fiss, 2009, 911; Lounsbury 2007). Yet, even as diffusion researchers have come to more of a consensus on questions of motivation, questions

about how firms assess economic or social value remain unsettled. Most diffusion studies continue to use density as a convenient measure of social contagion. Vicarious learning theorists have criticized this practice, arguing that appropriately specified models of social contagion must include the *results* of prior adopters (e.g., Strang and Macy 2001). Vicarious learning theory implies that imitation is conditional on performance feedback: firms only imitate practices that bring success for others, and avoid practices associated with failure (Cyert and March 1963; Ingram 2002; Strang and Macy 2001). Research has found some evidence for these propositions in contexts where the performance effects of adoption were large, reliable, and readily observable (e.g., Haunschild and Miner 1997; Jonsson 2009).

Yet, most contemporary diffusion studies do not control for the performance outcomes of prior adopters (cf. Dobbin, Simmons, and Garrett 2007; Strang and Macy 2001). This could be a product of data limitations: performance outcomes are often difficult to measure (Jonsson 2009, 179). But there are also strong theoretical reasons to believe that firms would not respond to the performance outcomes of other innovation users. Firms rarely share proprietary performance information with competitors, which makes it difficult for firms to systematically observe the precise results of innovation use elsewhere (Abrahamson and Rosenkopf 1993; Greve 2011). Some have argued that the unobservability of this information may explain why firms use industry prevalence to infer innovation value—this is the best indicator available (Abrahamson and Rosenkopf 1993; Greve 2011).

While it is true that firms in competitive markets rarely observe the precise effects of innovation use at other firms, I predict that they will still respond to available performance evidence. Firms almost always have access to information about the *general* performance of their publicly traded competitors, that is, performance on broad metrics like profitability or share price. Performance on these metrics may not be driven by the use of specific innovations, but these are the measures that firms can observe. When this is the performance information that firms have, I expect they will learn from it.

Specifically, I argue that decision-makers learn superstitiously from rough impressions of “how well recent innovation users are doing,” where “doing well” is proxied by overall bank performance. “Superstitious” learning occurs when actors attribute outcomes to a recent action that may not have caused the outcomes: in other words, firms may embrace an activity because it co-occurred with favorable performance, regardless of whether it actually contributed to performance (Levitt and March 1988, 326). Previous research finds that firms respond to success stories: when firms seek new strategies, they “follow the leader,” mimicking the strategies of profitable and powerful others (Haveman 1993; Strang and Macy 2001). I offer a related but alternative mechanism of social learning: when firms are faced with the decision to engage in a *particular* innovation, they “follow the outcomes” by responding to the performance of recent users on salient general metrics.

While others have not posited that corporations engage in superstitious vicarious learning, Lee and Strang (2006) have documented vicarious learning from general performance among national governments. In a study of policy diffusion

among OECD countries, they find governments more likely to downsize the public sector when downsizing elsewhere was followed by improvement on broad indicators like GDP growth, trade balance, and unemployment. The size of the public sector surely played but a small role, but the point is that organizations do not need objective evidence that the practice “caused” the outcomes to learn from the successes or failures of others. If decision-makers observe that members of a club have been successful, their desire to join may still increase even if they cannot be certain that membership was the cause.

This mechanism is consistent with anecdotal accounts of decision-making in the banking industry. Evidence from the business press implies that the fixed-income business in general, and the CDO underwriting business in particular, were widely regarded as important new strategic directions (e.g., Graubard 2001; Mackenzie 2004). Banks would know the extent to which competitors had embraced this strategy, and they could see how competitors were performing. Comments from one investment bank employee illustrate how this information could combine to influence decision-makers: “They [bank decision-makers] thought, ‘These guys [banks expanding into fixed income] seem to be making a lot of money, we should do that [expand our own fixed income business]’” (interview, New York, August 2013). The preceding discussion offers the following prediction:

H1: The higher the proportion of recent activity users that saw improved general performance, the more likely the focal firm is to engage in the activity.

Effects of Institutionalization

Prior research suggests that predictors of diffusion may change with the institutionalization of the practice (Tolbert and Zucker 1983; Kennedy and Fiss 2009). In what follows, I outline predictions for how effects of superstitious learning and direct experience will shift with the *industry utilization* of CDO underwriting. Industry utilization is analogous to industry density: both are indicators of an innovation’s social acceptance and institutionalization.¹

Practices are often theorized in terms of general models and causal relationships, and are more likely to spread quickly and widely when their theorized accounts fit with dominant understandings of rational and appropriate behavior (Davis and Greve 1997; Strang and Meyer 1993; Strang 2011). A theory-based learning perspective (Lee and Strang 2006; Strang 2011) suggests that vicarious learning is also contingent on the theorization of the practice. In an analysis of policy diffusion among OECD countries, Lee and Strang (2006) demonstrate that governments only learned from vicarious evidence that confirmed what was already theorized to be true. The dominant neoliberal ideology framed public-sector downsizing as effective, and public-sector upsizing as ineffective. Governments responded to vicarious evidence that confirmed prevailing theories (e.g., when good performance followed downsizing or when bad performance followed upsizing) but ignored contradictory evidence (e.g., when good performance followed upsizing or bad performance followed downsizing).

The theorization of the policy moderated what governments learned from the performance of others.

I combine insights from neoinstitutional diffusion theory and theory-based learning to argue that the effects of superstitious vicarious learning will change as an innovation becomes increasingly institutionalized. The theory-based learning perspective implies that firms are more responsive to confirmatory evidence when it is associated with an institutionally legitimate practice, or a practice that is theorized as effective (Jonsson 2009; Lee and Strang 2006; Strang 2011). Institutionalized practices, by definition, are theorized as both effective and legitimate: “appropriate and necessary component[s] of efficient, rational organizations” (Tolbert and Zucker 1983, 26). Accordingly, as the industry utilization of the innovation increases, I predict that firms will grow *selectively* more responsive to the performance outcomes of recent users. The cognitive biases associated with institutionalization will prompt actors to increase their focus on evidence that confirms what they already believe to be true—that the innovation is effective and worthwhile—and to reject evidence that contradicts this framing. The preceding discussion yields the following prediction:

H2: As industry experience with an activity accumulates, firms will grow more (less) responsive to good (poor) performance among recent activity users.

To isolate effects of social contagion, models of innovation engagement must account for direct experience with the diffusing innovation. Greater prior experience with a practice increases the likelihood that a firm will return to the practice (e.g., Amburgey and Miner 1992; Baum, Li, and Usher 2000). I posit that effects of direct experience will also vary with the institutionalization of the practice. Prior research demonstrates that the predictive power of organizational characteristics declines over the diffusion trajectory (Tolbert and Zucker 1983). Neoinstitutionalists have attributed this finding to the motives of early versus late adopters: early movers adopt to address organization-specific needs, while late-comers adopt more indiscriminately (DiMaggio and Powell 1983; Tolbert and Zucker 1983). Adoption motives change over time because external pressures to adopt increase with the practice’s popularity (Abrahamson and Rosenkopf 1993; Tolbert and Zucker 1983). I predict that the same mounting external pressures will make inexperienced users less hesitant to embrace the diffusing practice. This implies that direct experience will be less predictive of innovation engagement at higher levels of industry utilization.

H3: As industry experience with an activity accumulates, past experience with an activity will have less impact on the likelihood of reengaging in that activity.

Data and Method

This study includes data on every CDO underwritten ($N = 2,594$) by the entire population of US publicly traded commercial and investment banks active at any point between January 1, 1996, and March 31, 2007 ($N = 268$). I obtained CDO

underwriting data from SDC Platinum, a collection of financial databases produced by Thomson Reuters. These data include information on each CDO issuance, the date underwritten, the identities of the underwriter(s), and limited information on collateral and funding structure.

The analysis begins at Q1 1996, when the modern CDO market emerged,² and ends at Q2 2007, when CDO underwriting peaked and reports of problems in the housing market began to emerge. Coverage of CDO underwriting during this time period is comprehensive and reliable, and quality of data collection is consistent across all years.

Banks could underwrite CDOs individually or as part of a syndicate, a temporary group of underwriters that collaborates to bring an issuance to market. Syndicates underwrote 18 percent of CDOs in this sample. Each syndicate member received underwriting credit, regardless of role. Some banks underwrote multiple CDOs on the same day, which presents a problem for the estimation of ordered event data. To address this, I define an event as the act of underwriting *one or more* CDOs on a given day.

The term “bank” refers to all publicly traded firms operating within primary SIC codes 6211 (Securities Brokers and Dealers), 6282 (Investment Advice), or 6020 (Commercial Banks). CDO underwriters were sometimes subsidiaries of larger parent banks: subsidiaries do not report information on structure or financial performance. When a subsidiary (e.g., J. P. Morgan Securities) underwrote a CDO, I transferred underwriting credit to the parent bank (e.g., J. P. Morgan & Co.).

Independent Variables

I merged the CDO underwriting data with information on bank characteristics and performance from the COMPUSTAT database. The focus of this study is the CDO underwriting of publicly traded banks: banks enter the sample only after they go public, even if they previously underwrote CDOs as a private bank. If a focal bank was acquired during this time period, the acquirer received credit for its past activity.

The Banking Act of 1933 barred commercial banks from securities underwriting. This legal boundary began to erode in 1987, after regulators permitted bank holding companies (BHCs), on a case-by-case basis, to underwrite securities via a Section 20 subsidiary. The 1999 Gramm-Leach-Bliley Act allowed BHCs to reorganize as financial holding companies (FHCs) that could underwrite securities. A commercial bank is considered “at risk” of CDO underwriting after establishing a Section 20 subsidiary ($N = 34$) or reorganizing as an FHC ($N = 115$). Data on bank structure come from the Federal Reserve website.

Superstitious vicarious learning

I measure performance outcomes on two general metrics: return-on-assets (ROA) and share price. These performance measures are salient and readily observed, and among the most common measures of firm performance (Miller and Chen 2004; Baum and Dahlin 2007, 372). There is abundant evidence that banks actively seek to improve performance on both dimensions (e.g., Dobbin and Zorn

2005; Fombrun and Shanley 1990). I measure the post-CDO performance of other CDO underwriters [*Share price (underwriters)* and *ROA (underwriters)*] as the proportion of all CDOs underwritten by all other banks j during the most recent quarter that were also followed by improved share price (or ROA) for the underwriter. Since little is known about *how* firms actually learn from the performance outcomes of others (Strang 2011), I model performance outcomes as simply and broadly as possible: if more underwriters saw better (versus stable or worse) performance, was the focal bank more likely to underwrite CDOs? Mean quarterly change in underwriter share price (ROA) was not a significant predictor of CDO underwriting. I follow the vicarious learning literature and use recent, rather than cumulative, performance measures (Baum and Dahlin 2007; Haunschild and Miner 1997).

Industry utilization

I control for the industry utilization (institutionalization) of CDO underwriting [*Industry CDOs*], measured as the cumulative total of CDOs underwritten by all other banks j on or before time t .³

Direct experience

I control for the extent and positivity of a bank's direct experience with CDO underwriting. Extent [*Bank's CDOs*] is measured as the cumulative total of CDOs underwritten by the focal bank on or before time t . Positivity [*Share price (focal bank)*; *ROA (focal bank)*] is measured as the proportion of CDOs underwritten by the focal bank on or before time t that were followed by improvement in focal bank share price (or ROA). The positivity variables are poor proxies for the true performance effects of CDO underwriting. Banks likely knew how their own CDOs had performed, but this sort of fine-grained performance information was (is) unavailable to both researchers and competitors. Thus, the positivity variables actually test superstitious *experiential* learning: responding to one's own post-CDO general performance.

I also control for an interaction between the extent and positivity of a bank's direct experience [*Share price (focal bank)* \times *Bank's CDOs*; *ROA (focal bank)* \times *Bank's CDOs*]. Banks may be more responsive to their own post-CDO performance at higher levels of direct experience.

Interactions with industry utilization

I construct three interaction terms to assess how the effect of direct experience changed with the institutionalization of CDO underwriting. I interact each direct experience variable [*Bank's CDOs*; *Share price (focal bank)*; *ROA (focal bank)*] with industry utilization. To test whether the effects of superstitious vicarious learning changed with institutionalization, I also interact each underwriter performance variable [*Share price (underwriters)*; *ROA (underwriters)*] with industry utilization. I then disaggregate the underwriter performance variables into dichotomous "good" and "poor" performance variables. Reported models define good performance [*Good share price (underwriters)*; *Good ROA (underwriters)*] as underwriter performance at or above the 85th percentile of underwriter

performance, and poor performance [*Poor share price (underwriters)*; *Poor ROA (underwriters)*] as underwriter performance at or below the 15th percentile. Results are robust to alternative specifications of good/poor performance. I interact the good and poor performance variables with industry utilization to test whether banks became more responsive to confirmatory evidence and less responsive to contradictory evidence.

Controls

Performance controls

Banks likely compared the performance of CDO underwriters to the performance of banks that did not underwrite CDOs. All models that include the underwriter performance variables also control for non-underwriter performance that quarter, ensuring that effects of superstitious vicarious learning are net of general trends in bank profitability or share price. Non-underwriter performance [*Share price (non-underwriters)*; *ROA (non-underwriters)*] is measured as the proportion of all other non-underwriters during the most recent quarter that saw improved share price (or ROA).

Banks that never underwrote a CDO do not have a history of post-CDO performance, so I include a dichotomous “*Never underwrote*” variable (1 = no CDOs on or before time t) in models with the direct experience (positivity) variables. No bank underwrote CDOs in Q4 1996, so I include a “Q4 1996” dummy variable in models with the underwriter performance variables.

Bank characteristics

I control for bank *size* using the natural log of total assets, and I also control for a measure of bank profitability, ROA. Poor performance may drive banks to search for new strategies (Strang and Macy 2001) or, conversely, prompt banks to maintain the status quo (Staw, Sandelands, and Dutton 1981). Since bank reputation may affect the types of securities the bank can profitably underwrite (Podolny 1993), I control for bank status using a dichotomous *High status* variable (1 = bank was among top 50 global debt underwriters that quarter, 0 otherwise). Information on status was collected from Thomson Reuters league tables. League table rankings reflect market share, a conventional measure of underwriter status (Megginson and Weiss 1991). A continuous measure of bank status (league table ranking, reverse coded) was not statistically significant.

I use a dichotomous *investment bank* variable to control for bank type (investment or commercial), coded 1 if the bank primarily operated in the 6211 or 6282 SIC codes ($n = 139$), 0 otherwise ($n = 129$). Although Gramm-Leach-Bliley effectively eliminated boundaries between commercial and investment banking, differences in activities and regulation (including capital adequacy standards) persisted.⁴ I also control for two common predictors of bank risk: financial *leverage* (assets/shareholder's equity) and *z-score*, or each bank's distance from insolvency. Higher leverage and lower *z-scores* are associated with greater risk-taking. To calculate time-varying *z-scores*, I follow the approach of Lepetit and Strobel (2013, 75). A dichotomous “*First use*” variable isolates each bank's first event of

CDO underwriting, since the hazard of the first event may differ from that of subsequent events.

Market trends

Banks could also learn vicariously from an additional indicator of CDO market performance: the percentage of CDO tranches downgraded by credit ratings agencies. When CDOs overall performed poorly, banks may have curtailed their underwriting. All reported models control for the percentage of CDO tranches downgraded in the previous year [*Downgrade*]. I use data compiled by Benmél-ech and Dlugosz (2010, 177, table 5) to control for CDO downgrades. These data are the best available, but they have limitations: downgrades are reported yearly, and include only CDOs rated by Moody's. I also control for trends in industry risk tolerance, controlling for the mean financial leverage of all other banks [*Industry risk*]. Table 1 presents descriptive statistics for all covariates and controls; table 2 presents a correlation matrix. All controls and covariates are measured quarterly (except *Downgrade*) and lagged by one quarter.

Table 1. Univariate Descriptive Statistics (N = 8,373)

Variables	Mean	Std. dev.	Min	Max
Post-CDO performance				
Share price (underwriters)	0.586	0.341	0	1
ROA (underwriters)	0.444	0.266	0	1
CDO prevalence				
Industry CDOs (in 100 s)	11.160	7.581	0	27.380
Bank's CDOs (in 100 s)	0.499	0.917	0	4.240
Performance controls				
Share price (focal bank)	0.599	0.109	0	1
ROA (focal bank)	0.462	0.127	0	1
Share price (non-underwriters)	0.510	0.169	0.087	0.791
ROA (non-underwriters)	0.438	0.068	0.289	0.608
Never underwrote	0.622	0.485	0	1
Q4 1996	0.011	0.103	0	1
Bank and market controls				
Log size	9.127	3.739	-1.671	14.449
ROA	0.328	2.700	-23.674	9.970
Leverage	12.506	9.105	0	124.757
Z-score	11.107	14.869	0	144.474
High status	0.340	0.474	0	1
Investment bank	0.523	0.499	0	1
Downgrade	23.406	12.666	0	48
Industry risk	9.298	0.490	8.269	10.910

Table 2. Correlation Matrix

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
(1) Bank's CDOs	1.000								
(2) Industry CDOs	0.426	1.000							
(3) SP (focal bank)	-0.040	-0.042	1.000						
(4) ROA (focal bank)	0.030	0.116	0.325	1.000					
(5) SP (underwriter)	0.002	0.065	0.049	0.012	1.000				
(6) ROA (underwriter)	0.002	0.138	0.028	-0.016	0.374	1.000			
(7) SP (non-underwrit.)	-0.027	-0.006	0.022	0.002	0.652	0.212	1.000		
(8) ROA (non-underwrit.)	-0.079	-0.049	-0.009	0.014	0.182	0.295	0.210	1.000	
(9) Never underwrote	-0.697	-0.155	0.015	-0.025	-0.026	-0.056	0.298	0.429	1.000
(10) Q4 1996	-0.056	-0.152	-0.011	-0.010	-0.175	-0.176	-0.026	0.030	0.076
(11) Log size	0.639	0.204	-0.030	-0.004	0.018	0.042	-0.169	0.059	-0.788
(12) ROA	-0.020	-0.013	0.009	0.017	-0.027	-0.027	0.018	-0.018	0.018
(13) Leverage	0.498	0.061	0.074	-0.039	-0.006	0.026	-0.018	-0.012	-0.606
(14) Z-score	-0.211	0.051	0.007	0.041	-0.028	-0.011	-0.001	-0.030	0.307
(15) High status	0.742	0.149	-0.060	0.002	0.019	0.046	0.002	-0.036	-0.886
(16) Investment bank	0.045	-0.181	0.114	0.018	-0.040	-0.039	0.016	-0.026	0.010
(17) Downgrade	0.056	0.276	-0.010	0.056	0.138	0.171	-0.029	0.144	-0.040
(18) Industry risk	-0.109	0.064	-0.075	-0.052	-0.094	-0.060	0.081	0.075	0.147
	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	
(10) Q4 1996	1.000								
(11) Log size	-0.061	1.000							
(12) ROA	0.007	0.076	1.000						
(13) Leverage	-0.035	0.671	-0.063	1.000					
(14) Z-score	0.013	-0.403	0.302	-0.528	1.000				
(15) High status	-0.055	0.777	-0.026	0.647	-0.319	1.000			
(16) Investment bank	0.056	-0.282	0.010	0.005	0.319	0.028	1.000		
(17) Downgrade	-0.192	0.044	-0.099	0.003	-0.091	0.031	-0.151	1.000	
(18) Industry risk	0.047	-0.118	0.000	-0.186	0.050	-0.148	-0.107	0.151	

Analytic Approach

I use continuous-time, repeated-event event history methods to analyze bank engagement with CDO underwriting. Event history models estimate the hazard rate of event occurrence, or the probability per time unit that a subject (in this case a bank) at risk of an event (in this case underwriting CDOs) will experience

the event at time t . Event history models generate unbiased estimates when observations are censored, or when information about the subject (and whether/when it experienced the event) is incomplete or missing. I use a parametric event history model⁵ with an exponential distribution of duration time T . The baseline hazard of CDO underwriting is assumed to be constant across time and units, conditional on the covariates. I used the Akaike Information Criterion (Akaike 1974) to compare the fit of the exponential model against alternative distributions of T and found that the exponential model had the lowest AIC value, making it the preferred model.

Subjects are not dropped from the risk set after experiencing the event. When two or more events occur for the same subject, failure times are correlated, which violates the “independence of event time” assumption of survival analysis (Cleves 1999); thus, I use the “variance-adjusted” Andersen-Gill modeling framework (Andersen and Gill 1982), which treats each event as independent, conditional on variance-corrected errors and appropriately specified time-dependent covariates. This framework defines time-until-event as elapsed time from the onset of risk to the occurrence of the event, and the risk set is unrestricted. All models use bank-clustered robust standard errors.

Coefficients have a proportional hazards interpretation. I calculate the effect of each independent variable with the formula $100[\exp(\text{coefficient}) - 1]$, giving the percent change in the hazard of CDO underwriting that results from a one-unit increase in the given independent variable.

Findings

A baseline model assesses the effect of bank and market characteristics on the likelihood of CDO underwriting. Model 1 of table 3 demonstrates that larger banks, high-status banks, and investment banks were significantly more likely to underwrite CDOs, consistent with the findings of previous research. The positive and significant “ z -score” coefficient implies that riskier banks were less active underwriters, but the predominant pattern across all models is one of insignificant effects for both measures of bank risk (z -score and leverage). Similarly, while the CDO downgrade coefficient is positive (not significant) in the baseline model, the predominant pattern is one of negative and significant effects for this variable, implying that banks were more likely to underwrite CDOs after observing fewer downgrades. Only coefficients for size and status maintain significance across all models.

Model 2 introduces the direct experience controls. As expected, banks with a more extensive history of CDO underwriting were more likely to underwrite additional CDOs. However, the positivity of a bank’s post-CDO performance (as measured on general metrics) did not predict its future behavior. Banks did not learn superstitiously from their own general performance outcomes, possibly because they had more precise performance information.

Model 3 introduces the superstitious vicarious learning variables and the control for industry utilization. As expected, banks were more likely to underwrite CDOs when this practice was popular with others. However, the results for superstitious vicarious learning were somewhat unexpected. Banks were not

Table 3. Event History Estimates of CDO Underwriting, 1996–2007, Effects of Superstitious Learning

	M1	M2	M3	M4	M5
<i>Post-CDO performance:</i>					
Share price (underwriters)			0.207* (0.099)	0.222* (0.099)	0.218* (0.101)
ROA (underwriters)			−0.169 (0.103)	−0.190 (0.103)	−0.190 (0.101)
<i>Prevalence:</i>					
Industry CDOs			0.036* (0.017)	0.003 (0.013)	0.000 (0.012)
Bank's CDOs		0.539*** (0.086)		0.534*** (0.097)	0.272 (0.275)
<i>Performance controls:</i>					
Share price (focal bank)		0.499 (0.358)		0.432 (0.365)	0.322 (0.401)
ROA (focal bank)		0.038 (0.288)		0.033 (0.300)	0.029 (0.299)
Share price × Bank's CDOs					0.448 (0.447)
Never underwrote		−3.430*** (0.423)		−3.411*** (0.427)	−3.376*** (0.429)
Share price (non-underwriters)			−0.233 (0.202)	−0.281 (0.170)	−0.297 (0.175)
ROA (non-underwriters)			0.227 (0.734)	0.527 (0.664)	0.577 (0.667)
Downgrade	0.000 (0.004)	−0.005 (0.003)	−0.004 (0.004)	−0.007* (0.003)	−0.007* (0.003)
<i>Bank & market controls:</i>					
Log size	1.165*** (0.149)	0.498*** (0.107)	1.004*** (0.173)	0.495*** (0.104)	0.518*** (0.114)
ROA	−0.163 (0.144)	−0.057 (0.143)	0.005 (0.302)	−0.067 (0.137)	−0.091 (0.124)
Leverage	0.020 (0.018)	−0.003 (0.012)	0.024 (0.017)	−0.004 (0.012)	−0.003 (0.012)
High status	1.719*** (0.410)	1.326*** (0.294)	1.898*** (0.406)	1.333*** (0.289)	1.307*** (0.294)
Investment bank	0.597** (0.188)	0.290 (0.149)	0.368 (0.246)	0.292 (0.162)	0.276 (0.162)

(Continued)

Table 3. *continued*

	M1	M2	M3	M4	M5
Z-score	0.040*** (0.010)	-0.034 (0.028)	-0.009 (0.037)	-0.039 (0.035)	-0.034 (0.036)
First use	1.282*** (0.387)	1.466*** (0.345)	1.538*** (0.368)	1.587*** (0.362)	1.570*** (0.358)
Industry risk	-0.044 (0.076)	0.099 (0.101)	0.018 (0.092)	0.112 (0.101)	0.124 (0.101)
Log pseudo likelihood	8783.64	9126.51	8947.68	9133.55	9135.21

Note: Q4 1996 included in M3–M5, coefficient not reported. *p*-values based on bank-clustered robust standard errors. *N*(banks) = 268; *N*(CDOs) = 2,283; *N*(obs.) = 8,347.
p* < .05 *p* < .01 ****p* < .001 (two-tailed tests)

equally responsive to the two measures of CDO underwriter performance. The share price performance of other underwriters was a significant predictor of CDO underwriting: a one-tenth-unit increase in the proportion of CDO underwriters that saw improvement in share price (e.g., from 0.5 to 0.6) was associated with an average 2.5 percent increase in the hazard of CDO underwriting. However, the profitability (ROA) of other underwriters was not a significant predictor of CDO underwriting.

The results of model 4 confirm that direct experience does not “crowd out” the effects of superstitious vicarious learning, consistent with the results of previous research (Schwab 2007). Even after controlling for a focal bank’s direct experience with CDO underwriting, the share price performance of other CDO underwriters continued to predict engagement with this practice. However, the effect of industry utilization was no longer significant, which implies that increasing social acceptance alone did not drive the rise and spread of this practice.

After standardizing all variables in model 4, I find that direct experience was a more powerful predictor of innovation engagement than the observed experiences of others (results not shown). A one-standard-deviation change in direct experience was associated with a 1.453-standard-deviation change in the hazard of CDO underwriting; a one-standard-deviation change in the share price performance of other CDO underwriters was associated with a 1.084-standard-deviation change. Model 5 includes an interaction between a bank’s direct experience with CDO underwriting and its history of post-CDO share price performance; this interaction is positive but not significant.⁶ The implication is that banks were no more or less likely to learn superstitiously from their own post-CDO performance at higher levels of direct experience.

Table 4 reports how predictors of CDO underwriting shifted with the innovation’s institutionalization. Model 6 includes an interaction between the extent of a focal bank’s direct experience with CDO underwriting and accumulated industry experience with this practice. The results demonstrate that direct experience became less predictive of CDO underwriting as industry utilization increased.⁷ Figure 2 shows effects of direct experience at the mean level of industry utilization

Table 4. Event History Estimates of CDO Underwriting, 1996–2007, with Interaction Effects

	M6	M7	M8	M9	M10
<i>Post-CDO performance:</i>					
Share price (underwriters)	0.260** (0.095)	0.290** (0.093)	0.223* (0.097)		
ROA (underwriters)	-0.185 (0.104)	-0.095 (0.107)	-0.177 (0.100)	-0.169 (0.103)	-0.174 (0.110)
Share price × Industry CDOs		0.049*** (0.013)			
ROA × Industry CDOs			0.008 (0.018)		
Good share price (underwriters)				-0.065 (0.063)	
Good share price × Industry CDO				0.037** (0.012)	
Poor share price (underwriters)					0.189 (0.153)
Poor share price × Industry CDOs					0.001 (0.019)
<i>Prevalence:</i>					
Industry CDOs	0.009 (0.013)	0.004 (0.013)	0.003 (0.013)	0.006 (0.013)	0.002 (0.013)
Bank's CDOs	0.701*** (0.109)	0.552*** (0.093)	0.538*** (0.094)	0.528*** (0.096)	0.528*** (0.099)
Bank's CDOs × Industry CDOs	-0.022** (0.008)				
<i>Performance controls:</i>					
Share price (focal bank)	0.483 (0.356)	0.503 (0.356)	0.433 (0.365)	0.513 (0.347)	0.449 (0.361)
ROA (focal bank)	0.020 (0.286)	0.043 (0.295)	0.038 (0.300)	-0.012 (0.296)	0.024 (0.302)
Share price (non-underwriters)	-0.371* (0.162)	-0.525** (0.200)	-0.274 (0.176)	0.096 (0.098)	-0.069 (0.129)
ROA (non-underwriters)	0.090 (0.728)	0.413 (0.678)	0.533 (0.664)	0.261 (0.635)	0.318 (0.680)
Downgrade	-0.009** (0.003)	-0.010** (0.004)	-0.007* (0.003)	-0.006 (0.003)	-0.006 (0.003)
Q4 1996	-1.220 (0.730)	-1.635* (0.702)	-1.403* (0.709)	-1.583* (0.724)	-1.299* (0.645)
<i>Log pseudo likelihood</i>	9142.04	9146.73	9133.70	9136.35	9133.62

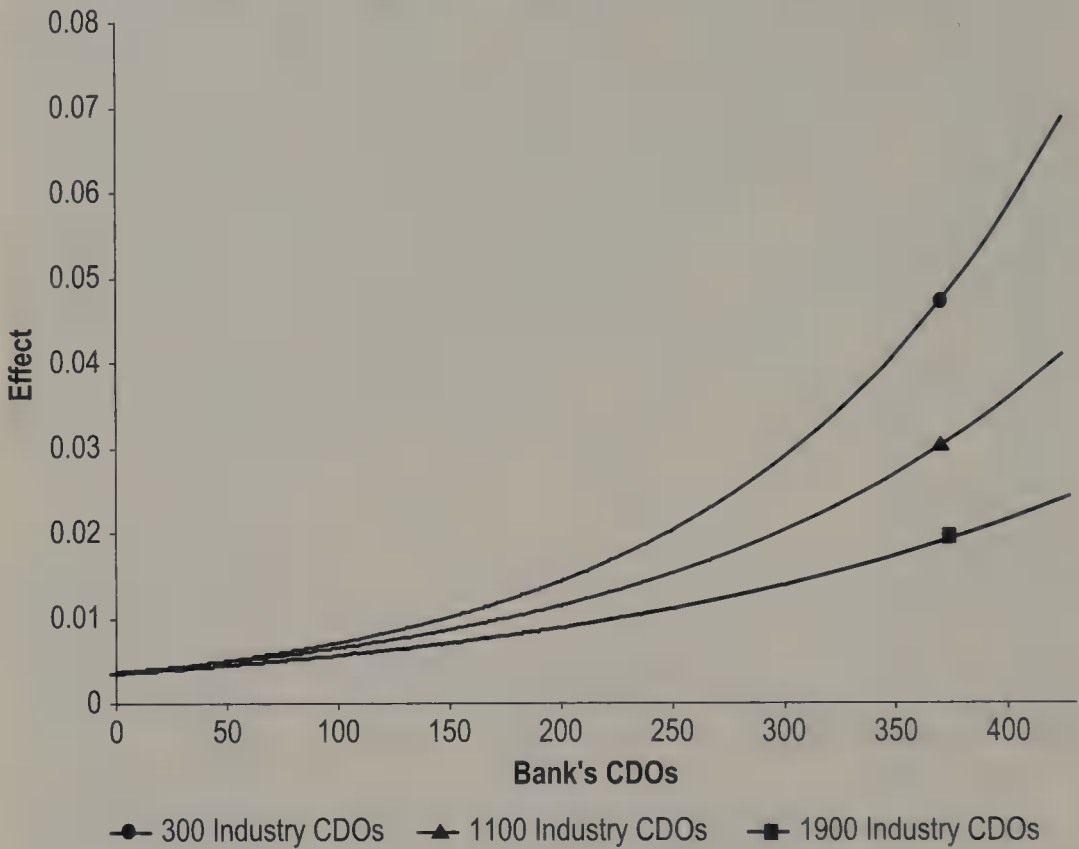
Note: Bank & market control variables included in all models, coefficients not reported.

p-values based on bank-clustered robust standard errors. *N*(banks) = 268; *N*(CDOs) = 2,283;

N(observations) = 8,347.

p* < .05 *p* < .01 ****p* < .001 (two-tailed tests)

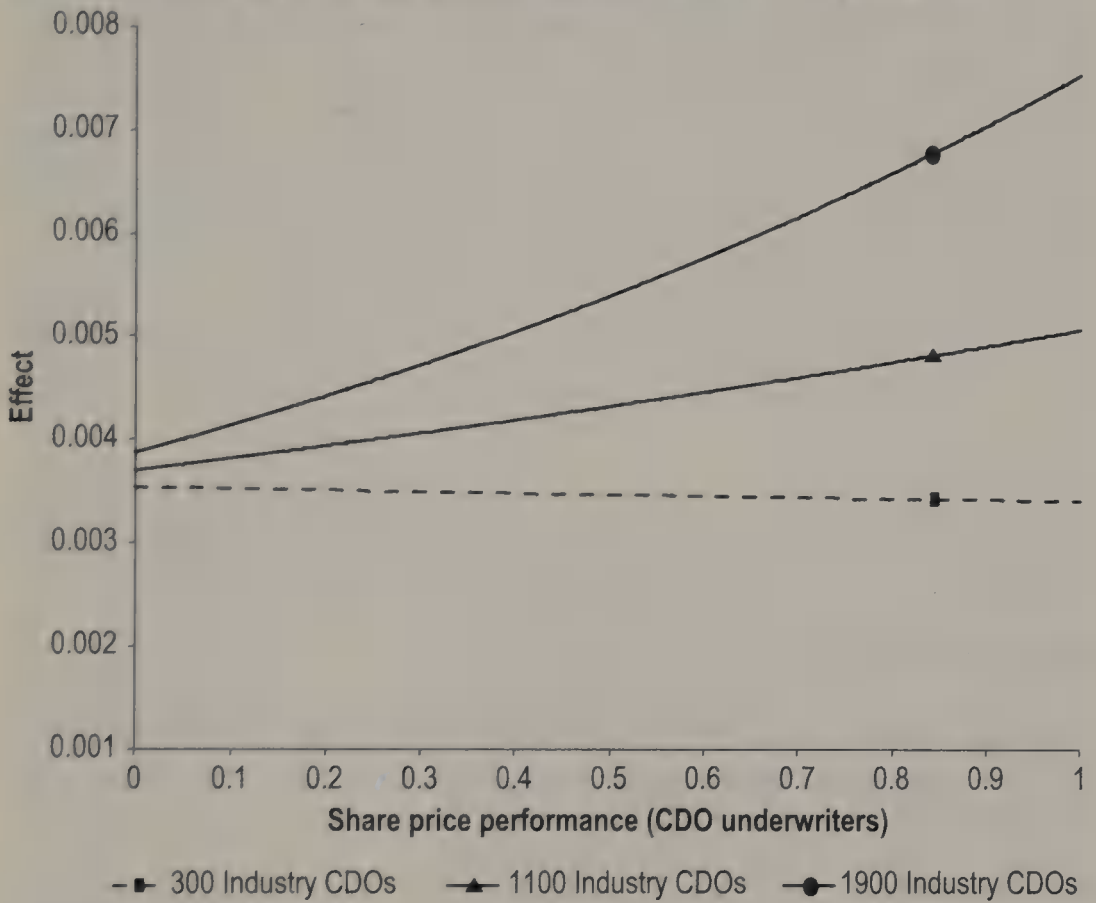
Figure 2. Conditional effect on CDO underwriting of ■ bank's CDOs at mean, mean + one standard deviation, and mean – one standard deviation levels of industry utilization



and at levels one standard deviation above and below the mean. These values correspond to 300, 1,100, and 1,900 CDOs underwritten by all banks, respectively. I use solid lines to indicate statistical significance at $p < .05$, and dashed lines otherwise. At the lowest levels of industry utilization, an increase in direct experience had the strongest effect for the hazard of CDO underwriting. An alternative interpretation is that more experienced CDO underwriters were less responsive to changes in industry utilization. However, results of post-estimation tests do not support this interpretation. After testing the effects of a nonlinear combination of industry utilization coefficients at each decile of the direct experience variable, I find that the effect of industry utilization was only significant ($p < .045$) at the lowest level of direct experience (e.g., zero CDOs). By contrast, the declining effects of direct experience were significant at every decile of industry utilization.

Models 7 and 8 include interactions between the superstitious vicarious learning variables and industry utilization. Model 7 demonstrates that banks became more responsive to underwriter share price performance as industry utilization increased. Figure 3 shows effects of underwriter share price performance at the mean level of industry utilization and at levels one standard deviation above and below the mean. At the lowest level of industry utilization, a one-unit increase in share price performance did not significantly increase the hazard of CDO underwriting. Effects at moderate to high levels of industry utilization ($N > 750$ CDOs)

Figure 3. Conditional effect of underwriter share price performance at mean, mean + one standard deviation, and mean – one standard deviation levels of industry utilization



were positive and statistically significant. Banks did not become more responsive to the ROA performance of other CDO underwriters (model 8).

Models 9 and 10 demonstrate that banks became *selectively* more responsive to the performance outcomes of other CDO underwriters. I predicted that banks would become more (less) responsive to good (poor) performance among other underwriters as industry experience with this practice accumulated. The results of model 9, which includes the *Good share price (underwriters)* dummy variable and its interaction with industry utilization, are consistent with this prediction: banks became more responsive to confirmatory evidence as the industry utilization of CDO underwriting increased. However, the results of model 10, which includes the *Poor share price (underwriters)* dummy variable and its interaction with industry utilization, were not consistent with this prediction: banks were no more or less responsive to contradictory evidence at higher levels of industry utilization.

Robustness Checks

Additional analyses evaluate the robustness of the findings to changes in CDO type, collinearity diagnostics, controls for large bank performance, and adjustment for selection bias.

CDO type

In the early 2000s, CDO issuers responded to problems in the market by changing the collateral mix of CDOs. They replaced corporate bonds, formerly the primary source of CDO collateral, with less volatile but more complex asset-backed securities (ABS) and other forms of structured finance. "ABS CDOs" were virtually unknown in 2000, but represented the majority of new issuances after 2002 (O'Leary 2002; MacKenzie 2011). Issuance of synthetic CDOs (e.g., CDOs backed by credit default swaps) also increased over time. I ran separate analyses predicting each type of CDO underwriting, and find that the same variables predicted bond/loan-backed CDO underwriting, ABS CDO underwriting, synthetic CDO underwriting, and all CDO underwriting. This reaffirms the appropriateness of including all types of CDOs in the analysis.

Multicollinearity

Collinearity diagnostics do not suggest a multicollinearity problem: condition indices of all models fall below 30 (mean condition index = 7.1), and variance inflation factors for all non-interacted variables fall below 10 (mean VIF = 2.6).

Large bank performance

The most prolific CDO underwriters were also among the largest banks; thus, banks may have responded to the share price performance of large banks, not that of recent underwriters. I reran all analyses with controls for the share price performance of the six largest banks; findings are robust to these controls.

Sample selection

The factors that led a bank to select into CDO underwriting (first-time adoption) might also lead the bank to underwrite CDOs more quickly and to a greater extent (subsequent engagement). Nonrandom selection into CDO underwriting may bias estimates of subsequent engagement. To account for the selection process that led some banks to underwrite at least one CDO, I used each bank's *propensity score* to reweight the sample (see Rosenbaum and Rubin 1983). Propensity scores reflect the probability of underwriting any CDOs, conditional on observed covariates. Observations with high scores were given more weight in CDO engagement models; observations with low scores were given less weight. This approach reduces bias by balancing nonequivalent comparison groups, adjusting for the relationship between observed covariates and selection into CDO underwriting. To estimate propensity scores, I used a probit model to predict the probability of selecting into CDO underwriting (1 = bank underwrote at least one CDO, 0 otherwise).⁸ Table 5 reports the results of the reweighted models, which demonstrate that key findings were robust to this adjustment for sample selection. Model 11 demonstrates that banks responded to the share price performance of other CDO underwriters. Model 12 shows that effects of direct experience declined with increasing industry utilization. Model 13 demonstrates that banks were more responsive to underwriter share price performance at higher levels of industry utilization. Models 14 and 15 confirm that banks grew more responsive to good, but not poor, performance on this dimension.

Table 5. Event History Estimates of CDO Underwriting, 1996–2007, Propensity Score Weighted

	M11	M12	M13	M14	M15
<i>Post-CDO performance:</i>					
Share price (underwriters)	0.235* (0.108)	0.281** (0.103)	−0.300* (0.142)		
ROA (underwriters)	−0.194 (0.100)	−0.190 (0.102)	−0.090 (0.101)	−0.166 (0.102)	−0.159 (0.101)
Share price × Industry CDOs			0.055*** (0.014)		
Good share price (underwriters)				−0.041 (0.054)	
Good share price × Industry CDOs				0.040** (0.013)	
Poor share price (underwriters)					−0.289* (0.142)
Poor share price × Industry CDOs					−0.018 (0.011)
<i>Prevalence:</i>					
Industry CDOs	0.024 (0.017)	0.037* (0.017)	0.025 (0.017)	0.026 (0.016)	0.019 (0.016)
Bank's CDOs	0.327** (0.100)	0.805*** (0.195)	0.346*** (0.096)	0.328** (0.103)	0.340** (0.109)
Bank's CDOs × Industry CDOs		−0.027** (0.009)			
<i>Performance controls:</i>					
Share price (focal bank)	−0.024 (0.450)	0.087 (0.420)	0.060 (0.435)	0.050 (0.427)	0.039 (0.448)
ROA (focal bank)	0.474 (0.280)	0.411 (0.267)	0.480 (0.270)	0.421 (0.284)	0.463 (0.277)
Share price (non-underwriters)	−0.269 (0.182)	−0.372* (0.188)	−0.549** (0.192)	0.105 (0.076)	−0.049 (0.124)
ROA (non-underwriters)	0.629 (0.752)	0.029 (0.777)	0.542 (0.776)	0.396 (0.724)	0.371 (0.753)
Downgrade	0.007* (0.004)	−0.010*** (0.003)	−0.011** (0.004)	−0.006* (0.003)	−0.007* (0.003)
Q4 1996	−0.991 (0.739)	−0.762 (0.732)	−1.266 (0.749)	−1.207 (0.763)	−0.995 (0.691)
<i>Log pseudo likelihood</i>	8103.45	8113.21	8116.62	8105.94	8105.095

Note: Time-varying propensity score model covariates were fixed at Q1 1998 values. *p*-values based on bank-clustered robust standard errors. Bank and market control variables included in all models, coefficients not reported. *N*(observations) = 5,271; *N*(banks) = 96; *N*(CDOs) = 1,918.

p* < .05 *p* < .01 ****p* < .001 (two-tailed tests)

Discussion and Conclusion

The objective of this article was to better explain the dynamics that drive the rise and spread of innovations, including socially undesirable innovations. What inspired commercial and investment banks to underwrite so many of the complex financial products that played such a prominent role in the credit crisis? Technological, economic, and regulatory changes made securitization increasingly attractive (Fligstein and Goldstein 2010; Carruthers 2010; MacKenzie 2011), but the way banks came to seize this profit opportunity provides an equally important, and underexplored, part of the story. I investigated whether banks learned superstitiously from loosely linked performance feedback, and examined how their responses changed with the increasing social acceptance of CDO underwriting.

The distinctive contribution of this study is its recognition that firms in competitive markets can and do respond to the general performance outcomes of other innovation users. I find that banks learned superstitiously from the share price performance of other CDO underwriters, escalating their engagement with CDO underwriting when recent underwriters seemed to be performing well, and deescalating their engagement when recent underwriters seemed to be performing poorly. As the practice of CDO underwriting became more prevalent in the industry, banks grew more attentive to evidence of good, but not poor, share price performance, and direct experience with CDO underwriting became less salient.

Contributions to Neoinstitutional Theory

Although these findings offer contributions to numerous theories of social contagion, including vicarious learning theory, they hold particularly important implications for neoinstitutional theory. The results suggest four important revisions to accounts of institutional effects in competitive markets. First, they imply that firms in competitive contexts do not simply imitate the behavior of their peers; instead, they learn vicariously from their general performance. Hundreds of diffusion studies have found that firms are more likely to adopt a practice when it is popular with others, yet these studies rarely control for performance outcomes. This raises the question of whether results traditionally attributed to the effects of institutionalization are actually products of superstitious learning. At the very least, future research should account for the general performance of prior adopters, not simply their number.

Second, while institutional pressures may not drive firms to blindly mimic popular practices, I find that institutionalization shapes the kind of evidence that catches their eye. As the industry utilization of CDO underwriting increased, banks became more attentive to confirmatory evidence, implying that institutionalization in competitive contexts operates through the mechanism of selective attention. This highlights opportunities for integrating neoinstitutional theory with other theories of social contagion. I show that the institutionalization of an innovation mediates what firms learn from its other users, uniting a theory-based learning perspective (Lee and Strang 2006; Strang 2011) and neoinstitutional diffusion theory. This finding also points to an area of mutual interest for

neoinstitutional theory and behavioral economics. Research in behavioral economics considers how confirmation bias operates to influence economic decision-making (Rabin and Schrag 1999; Fudenberg 2006); I find that the extent of confirmation bias is contingent on the institutionalization of the innovation. Future research might further explore how the activation of decision-making heuristics varies across institutional contexts.

Third, the results extend the neoinstitutional paradigm by demonstrating how firms integrate evidence from their own and others' experiences. Previous research on innovation abandonment finds that direct experience is an important check on blind imitation: if direct experience indicates that a popular practice is ineffective, firms tend to abandon it (Rao, Greve, and Davis 2001; Strang and Macy 2001; Greve 2011). My findings demonstrate that the reciprocal is also true: direct experience is less predictive of innovation engagement when an innovation is institutionalized, consistent with the argument that institutionalization increases the perceived economic and social benefits (costs) of embracing (avoiding) an innovation. Institutionalization alone does not explain bank involvement with CDO underwriting, but it may promote more indiscriminate engagement by decreasing the hesitance of inexperienced underwriters.

Fourth, these results imply that outcome salience may be institutionally contingent. Banks learned from the stock market performance of other CDO underwriters, but they did not respond to changes in underwriter profitability, although both performance measures were readily available. With the rise of the "shareholder value" model of corporate governance, share price has become the pre-eminent measure of corporate performance (Useem 1993; Lazonick and O'Sullivan 2000; Dobbin and Zorn 2005). In the late 1970s, institutional investors, securities analysts, and other groups who stood to benefit from changes in corporate structure selectively pushed firms to adopt remedies that would increase managerial accountability to shareholders (Useem 1993; Dobbin and Zorn 2005). One such remedy was stock option compensation, which redirects managerial focus toward increasing share price. This compensation scheme was widely adopted by publicly traded US firms (Hall and Liebman 1998) and became popular among banks (Chen, Steiner, and Whyte 2006). Since bank manager compensation increased when investors viewed the bank's activities favorably, managers were likely to be highly attuned to market perceptions. At the same time, a wave of corporate accounting scandals had increased distrust of ROA and other accounting-based performance measures. Thus, the institutional salience of the performance measure may explain when and whether firms learn from others (Lounsbury 2007). Future research might examine whether outcome salience varies across countries and industries, between publicly traded and private firms, or before and after transitions to new models of corporate governance.

Implications for Non-Corporate Contexts

An additional promising extension of this work might investigate how superstitious vicarious learning operates in contexts where financial performance is either unobservable or irrelevant. Governments, nonprofits, educational institutions, and privately held firms do not care about boosting stock price, but they do care

about performance on other indicators. Universities seek to increase enrollment, alumni giving, or position in external rankings; nonprofits compete for large external grants and community visibility. State governments can see whether others have balanced the budget or increased revenues.

Banks and the Credit Crisis

These results also contribute to a better understanding of bank behavior at a critical juncture. Bank involvement with CDOs amplified the effects of the credit crisis, not just in the United States, but worldwide (cf. Goddard, Molyneux, and Wilson 2009). CDOs had extraordinary consequences, so it is not surprising that most explanations have focused on the unique characteristics of this product: its unprecedented technical complexity, the role of the rating agencies, the inclusion of mortgages, and so on. But bank behavior can also be understood as quite conventional organizational behavior, conforming to well-known models. In many respects, CDO underwriting was just another financial innovation that diffused through ordinary organizational processes.

The focus on ordinary organizational processes in the CDO market highlights an overlooked explanation for the rise and spread of this practice. Conventional accounts have largely glossed over banks' motivations for underwriting CDOs: the received wisdom is that banks underwrote CDOs because they saw that they could make money. The account presented here suggests that the conventional wisdom is only partially true. Banks concentrated on a particular way of making money—improving share price—and they grew increasingly responsive to positive, but not negative, evidence on this dimension.

CDO underwriting became identified as a hot new strategic direction around the same time that banks began to report record profits and stock market returns. This connection could have been entirely coincidental; banks were engaged in a number of activities that might have led to improved share price. However, it is possible that the investment community directly rewarded banks that underwrote CDOs. Evidence from the business press suggests that investors were equally enthralled by the promises of complex securitization (Gregory 2001; Mullin 2002; Mackenzie 2004), and research in other domains finds that investors and analysts attend to changes in corporate practices, and reward firms that engage in behaviors seen as legitimate or effective (Zuckerman 1999; Zajac and Westphal 2004).

Regardless, the findings imply that as long as this practice was considered legitimate or effective, and as long as the banks that engaged in this practice saw better stock market performance, internal market dynamics alone could have driven the rapid rise in CDO underwriting between 2003 and 2007. A natural follow-up question is why banks were so focused on the stock market performance of their peers. Previous research implies that the extreme focus on share price is not the product of an inevitable confluence of market forces, but the outcome of a particular institutional arrangement (Useem 1993; Lazonick and O'Sullivan 2000; Dobbin and Jung 2010). Before the late 1970s, most investment banks were still privately held partnerships, and most commercial banks still engaged in traditional banking. But as the shareholder value model of corporate

governance gained traction in both industrial and financial circles, firms began to implement compensation practices that rewarded a single-minded focus on share price, and pursued riskier strategies to generate larger returns (Chen, Steiner, and Whyte 2006; Dobbin and Zorn 2005). Six years out from the crisis, these practices and strategies have hardly changed, prompting the question of what will happen the next time a hot new innovation coincides with strong market performance. As policymakers develop remedies designed to prevent the next credit crisis, they might pay more attention to what banks learned during the last one.

Notes

1. My goal is to explain the rise and spread of a repeatable innovation, or a practice that the same firm can embrace multiple times. When the innovation is repeatable, dropping firms after the initial use of the innovation can exclude potentially valuable information. Consider the case of CDO underwriting: only 36 of 268 publicly traded US banks that were eligible to underwrite a CDO elected to do so, yet these banks underwrote a combined 2,594 issuances. To understand how CDOs traveled from virtually unknown to pervasive over the course of a decade, it is important to consider a bank's entire history with this practice. I examine both initial and subsequent instances of CDO underwriting and use the shorthand "innovation engagement" to refer to this outcome. Social contagion (i.e., the observed examples of others) can inform both adoption and engagement decisions.
2. An alternative strategy is to begin the analysis in 1987, but early CLOs and CBOs were different from the CDOs that followed (Tavakoli 2003). Between 1987 and 1995, average CDO issuance averaged \$2 billion and never exceeded \$4 billion; in 1996, new issuance rose to \$38 billion (Lucas, Goodman, and Fabozzi 2006). Only 16 CDOs were issued before 1996.
3. Findings are also robust to substituting a quarterly (non-cumulative) measure of industry utilization. I use empirical comparisons to adjudicate between alternative specifications of effect timing, comparing models on the Akaike Information Criteria (Akaike 1974).
4. I also control for regulatory capital (Tier 1 & 2) in unreported analyses that exclude investment banks. Lower regulatory capital was a significant predictor of CDO underwriting only in the baseline model.
5. Parametric models may be more efficient than the Cox proportional hazard model (which does not constrain the functional form of T) if the data include intervals in which one or fewer subjects experienced the event. These data include such intervals. Unreported analyses demonstrate that estimated coefficients of the Cox model are substantially similar to those of the exponential model, implying that the underlying baseline hazard is not misspecified (Cleves, Gould, and Gutierrez 2004: 200–202).
6. An interaction between direct experience and a bank's post-CDO ROA performance was not statistically significant.
7. Interactions between the positivity of direct experience and industry utilization were not statistically significant.
8. I include the following covariates in the propensity score equation: *log size*, *ROA*, *leverage*, *z-score*, *investment bank*, *high status*, *industry risk*, *industry CDOs*, *share price (underwriters)*, *ROA (underwriters)*, *share price (non-underwriters)*, *ROA (non-underwriters)*. Estimating the outcome models required time-invariant propensity score weights. To create these weights, I fix time-varying predictors of selection at Q1 1998 values.

About the Author

Kim Pernell-Gallagher is a PhD candidate in sociology at Harvard University. Her ongoing research examines the organizational processes and institutional conditions that facilitate undesirable organizational behavior, especially in financial markets. Her dissertation investigates the historical origins of the divergent development of banking regulation, 1988–2007, in the United States, Canada, and Spain—three countries that subscribed to the same international regulatory standards.

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Status Beliefs and the Spirit of Capitalism: Accounting for Gender Biases in Entrepreneurship and Innovation

Sarah Thébaud, *University of California—Santa Barbara*

In this article, I develop and empirically test the theoretical argument that widely shared cultural beliefs about men's and women's abilities in entrepreneurship (i.e., "gender status beliefs") systematically influence the social interactions during which an entrepreneur, particularly an innovative entrepreneur, seeks support from potential stakeholders for his or her new organization. To evaluate this argument, I conducted three experimental studies in the United Kingdom and the United States in which student participants were asked to evaluate the profiles of two entrepreneurs and to make investment decisions for each. The studies manipulated the gender of the entrepreneur and the innovativeness of the business plan. The main finding is consistent across studies: gender status beliefs disadvantage typical women entrepreneurs vis-à-vis their male counterparts, but innovation in a business model has a stronger and more positive impact on ratings of women's entrepreneurial ability and overall support for their business ideas than it does for men's. However, the strength of these patterns varies significantly depending on the societal and industry context of the new venture in question. Findings indicate that gender status beliefs can be understood as an important "demand-side" mechanism contributing to gender inequality in aggregate entrepreneurship rates and a micro-level factor affecting the likelihood that a new and novel organization will emerge and survive.

A growing body of scholarship documents the prevalence of unconscious gender biases in modern work organizations. For instance, women managers are often believed to be less achievement oriented ("agentic") and competent than their male counterparts, which can result in women being given fewer rewards and held to a stricter standard of performance (Foschi 1996; Heilman 2001; Ridgeway 2011). Organizational efforts to prevent discrimination are also often unsuccessful and may even produce the opposite of their intended outcome (Castilla and Benard 2010).

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In light of these findings, it is not surprising that scholars and women business owners alike often cite entrepreneurship as a career in which women may be able to mitigate exposure to bias (Heilman and Chen 2003; Mattis 2004; Moore and Buttner 1997). This may be possible given that entrepreneurs have greater autonomy over their work environment and are not embedded in a preexisting set of organizational roles, each of which may be attached to gender stereotypes about performance and behavior (Reskin and Roos 1990; Ridgeway 2011). Entrepreneurs also typically hold less supervisory authority than managers do, a structural position that provokes dislike and derogation toward women (Rudman et al. 2012). However, a number of recent studies suggest that women entrepreneurs are not immune to gender bias: lenders, potential lenders, and technology licensing officers have all been found to favor male-owned start-ups (Bigelow et al. 2014; Brooks et al. 2014; Shane et al. 2012).

Perhaps because the bulk of prior research has focused on explaining patterns of gender bias *within* established organizational contexts (whether hypothetical or real), the social psychological mechanism responsible for bias in entrepreneurship contexts has yet to be fully developed or evaluated. Understanding this mechanism is important because women are even more underrepresented among entrepreneurs than they are in wage and salaried leadership positions. As recently as 2009, US women constituted about 43 percent of managers, legislators, and senior officials (UNDP 2009), yet they were majority owners of only 28 percent of all private firms (CWBR 2009). Men also run larger, more innovative, and more growth-oriented enterprises than their female counterparts (Loscocco and Bird 2012; Kalleberg and Leicht 1991; Tonoyan and Strohmeier 2005). Most accounts for this inequality have focused on “supply-side” factors, such as gender differences in network resources, financial means, managerial experience, or perceptions about the abilities and risks involved in starting a business (Loscocco et al. 1991; Marlow and McAdam 2010; Minniti and Nardone 2007; Renzulli, Aldrich, and Moody 2000; Thébaud 2010). Yet, substantial gender gaps persist after taking into account many such differences. By specifying a mechanism that underpins gender bias in entrepreneurship, it is thus possible to identify the sorts of social contexts in which this “demand-side” process is most likely to fuel gender-unequal entrepreneurship outcomes.

Understanding bias in entrepreneurship contexts is also important because, in contrast to many other careers, entrepreneurial success is uniquely contingent upon evaluative social interactions: an entrepreneur’s motivation and the organization’s survival ultimately depend on his or her ability to gather support from others. Gaining support for a new venture is difficult given that, to a certain degree, all new organizations lack legitimacy (Aldrich and Ruef 2006; DiMaggio and Powell 1991; Suchman 1995). Whereas most entrepreneurs overcome this liability by introducing organizations that largely mimic existing organizational forms and practices, those who challenge taken-for-granted practices by introducing novel products or processes must work extra hard to convince others that their ideas are viable (Aldrich and Fiol 1994; Schumpeter 1961[1934]; Suchman 1995). During this critical “local validation” (Johnson, Dowd, and Ridgeway 2006) stage, local actors serve as the gatekeepers of new ideas.

With the exception of some studies highlighting founders' strategic use of networks and communication (Baron and Markman 2003; Lounsbury and Glynn 2001), most theory and research on organizational legitimacy has focused on organizational-level processes (Johnson, Dowd, and Ridgeway 2006), and as a result tends to be disembodied from individual attributes. Yet, in early stages, entrepreneurs represent new organizations and the ideas behind them. Because cultural beliefs about gender are themselves widely legitimated, taken for granted, and relevant across many task-oriented settings (Ridgeway 2011), might they also influence the likelihood that a novel organization will be deemed worthy of support?

Thus, the goals of this research are twofold. First, I propose and test a theoretical mechanism that may be responsible for "demand-side" biases contributing to women's underrepresentation in entrepreneurship. Drawing on theory in the social psychology of gender and studies documenting the gendered context of entrepreneurship, I argue that "gender status beliefs"—widely shared cultural beliefs that generally confer men greater ability at the things that "count" in society—affect the way that others evaluate a potential entrepreneur's business idea. The patterns of gender-biased feedback that status beliefs generate may, in the aggregate, discourage women from persisting toward an entrepreneurial career and disadvantage them in their quest for social and financial support from potential stakeholders, who may include colleagues, family members, friends, investors, future customers and employees, or representatives of other organizations. However, per the scope conditions of the theory, the relative impact of these beliefs will vary according to the gender composition of entrepreneurs and managers in a society, as well as the gender composition of an industry.

Second, I advance theories of organizational legitimacy by investigating the extent to which gender status beliefs affect the likelihood that an innovative, as opposed to a conventional, business model will be perceived as worthy of support. If status beliefs inform the interactions through which entrepreneurs garner encouragement and support for their ideas, then the socially selective process that determines which new and novel organizations will survive and which will fail operates differently depending on the gender of the individual proposing it.

In the following sections, I elaborate my argument about the role of gender status beliefs in organizational creation and generate a series of propositions about the effects of gender and innovation on the likelihood of gaining support for a new venture. I then consider how my propositions may be moderated when gender status beliefs are likely to differ in their relevance to the entrepreneurship setting, either because 1) the gender composition of entrepreneurs and managers in a given society differs, or 2) the gender composition of entrepreneurs in a given industry differs. Finally, I evaluate my claims with three laboratory experiments that I conducted in the United Kingdom and the United States and conclude with a discussion of the theoretical contributions of this research.

Gender Status Beliefs in Entrepreneurship

Both survey and experimental studies indicate that men are often believed to be more competent and/or agentic than women (Correll and Ridgeway 2003; Fiske

et al. 2002; Koenig and Eagly 2014). For instance, Fiske et al. (2002) found that diverse groups of US respondents rated men higher than women on a scale that included perceptions of competence, intelligence, confidence, competitiveness, and independence. Experimental studies similarly find that gender, often unconsciously, cues expectations of competence in task-oriented situations (Correll and Ridgeway 2003), even when actors consciously express gender-egalitarian beliefs and intentions (Rashotte and Webster 2005).

More specifically, gender is understood to be a *status characteristic*, a categorical distinction based on either a personal attribute (e.g., gender, race) or a role (e.g., manager) that has attached to it widely shared cultural beliefs about the status worthiness of one category over the other (Berger et al. 1977). When effectively salient, status characteristics can influence behaviors and evaluations because they inform performance expectations regarding an individual's level of ability (e.g., competence) and/or effort (e.g., commitment) (Correll, Benard, and Paik 2007; Correll and Ridgeway 2003). Because they are expected to have more ability and exert more effort, high-status actors are given more opportunities to participate, have more influence over others, and have their performances evaluated more positively than low-status actors. A status characteristic is salient when it differentiates actors, or when it is believed to be relevant to the task at hand. For example, gender status beliefs are especially likely to inform performance expectations for particularly male-typed tasks (Ridgeway 2011).

Research widely confirms that entrepreneurship is one such male-typed task (Bird and Brush 2002; Buttner and Rosen 1988; Bruni, Gherardi, and Poggio 2004; Gupta et al. 2009). For instance, Gupta et al. (2009) found that business students in the United States, India, and Turkey strongly associate entrepreneurship with “masculinity” and stereotypically masculine traits. Moreover, characteristics stereotypically associated with entrepreneurship (e.g., willingness to take risks, competitiveness, aggressiveness, leadership ability, business sense) are not only perceived to be more typical among men, but are also seen as more desirable in men (Prentice and Carranza 2002). Therefore, when men become entrepreneurs, they fulfill stereotypes not only about how they are, but also about how they *should* be.

Unlike most management situations, entrepreneurship is also fraught with uncertainty regarding the probability of success. Research has shown that people are especially likely to rely on stereotypes in situations characterized by uncertainty and a lack of information (Gorman 2006; Ridgeway 2011).

Taken together, this literature suggests that gender will be salient as a status characteristic in entrepreneurship. That is:

H1: On average, women entrepreneurs will receive lower ability and effort ratings, and their businesses will be rated less worthy of support than men's, all else equal.

Importantly, if gender is salient as a status characteristic, then ratings of ability and effort should mediate gender differences in business support (e.g., evaluators will offer less investment to a woman entrepreneur *because* they believe her to be less competent than her male counterpart).

Gender Status Beliefs and Innovation

Next, scholars have noted that organizations fall on a continuum between the two poles of “reproducer” and “innovator” (Aldrich and Ruef 2006, 67). The vast majority of organizations are reproducers, with routines and competencies that largely mimic existing organizations. In contrast, innovator organizations depart from the standard way of doing things by, for instance, introducing new products/services, methods of production, or markets (Schumpeter 1961[1934]). In this section, I theorize how organizational innovations like these may moderate the effect of gender status beliefs on evaluations of entrepreneurs and their businesses. To do so, it is necessary to consider 1) how status beliefs may affect the standards used to evaluate the quality of a business idea, and 2) how organizational innovation may have bearing on men’s and women’s likelihood of gaining support.

To begin, research suggests that status characteristics, when salient, inform not only expectations of competence, but also the standards that are used to determine whether a task performance is indicative of ability (Correll, Benard, and Paik 2007; Foschi 1996). Specifically, as lower-status group members, women tend to have their performances judged by a stricter standard than men because when women perform a male-typed task well, their performances are inconsistent with expectations for women in general (Foschi 1996; Foschi, Lai, and Sigerson 1994). As a result, their performances are often more highly scrutinized, such that women must demonstrate more “evidence” of ability than their male counterparts in order to have their performances judged to be of the same quality. Thus, in the entrepreneurship setting, women entrepreneurs may need to demonstrate more evidence of entrepreneurial ability than their male counterparts in order for their business to be perceived as being equally worthy of support.

What signals entrepreneurial ability? In addition to human capital such as management, industry, or prior start-up experience, factors associated with organizational survival that are typically theorized at the organizational level, such as innovation, may signal an entrepreneur’s ability given that, in the early stages, individual entrepreneurs effectively embody new organizations and the ideas behind them. Organization theorists argue that innovator organizations tend to encounter more social resistance than reproducers (Lounsbury and Glynn 2001; Knudsen and Swedberg 2009; Schumpeter 1961[1934]; Sine, Haveman, and Tolbert 2005). This occurs because organizations that introduce new products or processes lack *cognitive* legitimacy: they are, by definition, not yet a taken-for-granted feature of the social environment (Aldrich and Ruef 2006; Aldrich and Fiol 1994; Suchman 1995). Producers, consumers, and other potential stakeholders have a relative lack of knowledge about the organization’s activities and its products/services, and are therefore uncertain about its probability of success. This heightened uncertainty and risk raises doubt about a new venture, which may lead to financial and/or social penalties. For instance, innovative entrepreneurs may be viewed as foolish to try something so risky (Aldrich and Fiol 1994). By the same token, there are tangible rewards for following convention: new organizations that conform to the structures and ceremonial activities of established firms in their industry are more likely to survive and grow (Khair 2010; Singh, Tucker, and House 1986).

If one considers the greater uncertainty and risk associated with innovation together with the idea that women are coded as a lower-status group, it suggests a double disadvantage for women: individuals whose performances are already more likely to be scrutinized may be at an even greater disadvantage when starting an innovative organization because innovation is also more subject to scrutiny. That is, membership in a lower-status category may serve to further undermine the credibility of an innovative entrepreneur, which is already in question by virtue of their departure from accepted practices. This leads to the expectation that:

H2a: Innovation will be more negatively associated with ability, effort, and business support ratings for women than men entrepreneurs, all else equal.

Research also suggests, however, that regardless of whether the social environment is relatively risk averse or risk tolerant, the comparatively greater risk and uncertainty associated with innovation typifies the gendered stereotype of an “entrepreneur.” That is, by implicitly being willing to take on more risk, *innovative* entrepreneurs exaggerate the character traits that are part and parcel of the ideal-typical cultural image of the entrepreneur: someone who is willing to buck norms, agentic, independent, competitive, risk tolerant, and competent. As noted earlier, this image is implicitly masculine because it is consonant with stereotypes about the kinds of traits men supposedly have and *ought to* have.

Because stereotypes about women don’t fit this image, women entrepreneurs may be viewed as more authentically “entrepreneurial” when they propose an innovative idea than when they propose a conventional one. In effect, innovation may signal the additional “evidence” of ability that double standards theory suggests women would need in entrepreneurship contexts. By better fitting the masculine image of the entrepreneur, innovative women may be viewed as more credible and thus more competent entrepreneurs. This dynamic may, paradoxically, mitigate or even override the skepticism that an innovative idea might otherwise invoke. By contrast, innovation may not play into evaluations of men’s entrepreneurial ability in the same way because their ability to be an entrepreneur more generally is less subject to scrutiny: by virtue of being a man, both innovative and non-innovative men entrepreneurs, to a certain extent, live up to stereotypes about how entrepreneurs are and should be. This leads to the competing expectation that:

H2b: Innovation will be more positively associated with ability, effort, and business support ratings for women than men entrepreneurs, all else equal.

Finally, if innovation differentially impacts ratings of men’s and women’s businesses *because* gender is salient as a status characteristic, then ability and effort ratings should mediate the interaction effect between gender and innovation.

Contextual Factors

As discussed, an important scope condition of status characteristics theory maintains that gender will be salient as a status characteristic in settings where the task

(e.g., entrepreneurship) is male typed. Thus, the extent to which this scope condition holds likely varies according to the extent of men's overrepresentation among entrepreneurs in a particular setting. I argue that such overrepresentation may occur along two dimensions: 1) men may be more or less overrepresented among entrepreneurs and managers in a given society, and 2) men may be more or less overrepresented among entrepreneurs in a given industry.

Societal Context

Operationally, the first factor can be informed by the gender composition of entrepreneurs and managers at the societal level. In particular, one can expect entrepreneurship to be less strongly male typed in contexts where women are more highly represented in these areas of the labor market. Because entrepreneurship is less male typed in such contexts, gender should be relatively less salient as a status characteristic in entrepreneurial evaluations. This means that the baseline status belief about men's greater ability in entrepreneurship should be relatively weaker, and as a result, the interaction between innovativeness and gender of entrepreneur should be weaker. In short,

H3: There will be weaker evidence for H1 and H2 in a societal context where women are more highly represented among entrepreneurs and managers.

To gain variance on macro-level inequality, I employ comparative case logic to develop a UK\US comparison. The US offers a robust comparison to the UK because it allows me to "hold constant" some basic attributes of political and economic systems, while providing variance on gender inequality in the labor market. In particular, the UK and US are similar in their levels of economic development, "liberal" capitalist models, and shared Anglo-Saxon cultural history (Esping-Anderson 1990; O'Connor, Orloff, and Shaver 1999). Laws pertaining to business start-up (World Bank Group 2010) as well as rates of entrepreneurship (Kelley, Bosma, and Amorós 2010) are also similar.

Yet, women's representation in entrepreneurship and management varies between the two contexts. First, women constitute a lower share of start-up activity in the UK (Kelley, Bosma, and Amorós 2010), where 15 percent of businesses are majority female owned (ISBE 2009), as compared to 28 percent in the United States (CWBR 2009). Second, women's representation in managerial positions is lower in the UK than in the US (Mandel and Semyonov 2006; Pettit and Hook 2009). These patterns may emerge in part from differing policies and cultural attitudes. For instance, UK mothers have access to longer periods of leave and better part-time employment opportunities, both of which can limit career prospects by interrupting and/or decreasing the likelihood of full-time employment (Gornick and Meyers 2009; Mandel and Semyonov 2006; Pettit and Hook 2009). There is also stronger ideological support for mothers' full-time employment in the US than in the UK (Treas and Widmer 2000; Treas and Tai 2011).

Notwithstanding these potential sources of variation, it suffices to say that because entrepreneurship and management are less male dominated in the United

States, gender should be relatively less salient as a status characteristic for entrepreneurs in a US setting than in a UK setting.

Industry

The second contextual factor that may affect the salience of gender as a status characteristic in entrepreneurship is the gender composition of the industry. Sex segregation by industry and occupation are widespread (Charles and Grusky 2004) and carry over into entrepreneurship, with women entrepreneurs concentrated in lower-profitability industries such as retail, food service, and interpersonal care (Loscocco and Bird 2012; Loscocco et al. 1991; Moore and Buttner 1997). Thus, entrepreneurship can be expected to be a more strongly male-typed task in male-dominated industries, especially those that draw on male-typed skills such as engineering. In these contexts, status beliefs about women's abilities in entrepreneurship are compounded with status beliefs about their abilities in other male-typed domains. By comparing industry contexts, it is possible to evaluate the extent to which any gender effects that emerge may be attributed to the salience of entrepreneurship as a male-typed task independent of the male-typed occupations and industries that are often endemic to it. Therefore, I propose that:

H4: There will be stronger support for H1 and H2 in a male-dominated industry that requires male-typed skills than in a gender-neutral industry.

Method

To evaluate my hypotheses, I conducted three experimental studies. Study 1 evaluates the effects of gender and innovation (H1 and H2) in a gender-neutral industry in a UK setting. Study 2 evaluates these same effects in a US setting, thus generating a comparison to Study 1 (H3). Finally, Study 3, also in a US setting, evaluates these effects in a high-tech industry, providing a comparison to Study 2 (H4).

Laboratory experiments are advantageous for evaluating cognitive biases because they provide a highly controlled setting in which I can obtain a diverse set of outcome measures. Moreover, factors that might otherwise interfere with hypothesis testing are absorbed through randomization. The key benefit of this approach is that it allows me to test the mechanism behind gender bias in entrepreneurship. Understanding this mechanism is important if a goal is to find ways to reduce the biases that women entrepreneurs have been found to experience.

In total, there were 178 student participants (21–41 per condition). Each study was conducted at a large research university ranked in the top tier of universities in its country. Studies 2 and 3 were conducted at the same university in the Northeast United States. Participants represented a wide range of majors, including arts and sciences, business, and engineering. The average age was 20 (standard deviation = 1.9), and there were 86 male and 92 female participants. Across the three studies, gender of participant did not significantly affect results; therefore, I do not discuss it further.

These participants offer a useful test of my propositions for a number of reasons. To begin, I theorize that gender status beliefs systematically influence the

way the average person reacts to new business ideas. If this is the case, women are less likely to receive positive feedback and support for a business idea and more likely to be discouraged. A study based on students serves as a first step toward evaluating this general social process. Students may also offer a conservative test of my hypotheses given that younger, university-educated people in both countries express more progressive gender ideologies (Bolzendahl and Meyers 2004; Knudsen and Waerness 2001). And, with the recent rise of entrepreneurship programs and competitions on college campuses, students increasingly have opportunities to weigh in on new ventures (*Entrepreneur* 2013).

Students are limited, however, in that they are not trained to evaluate business proposals. Therefore, they may have a greater tendency to react on the basis of stereotypes than individuals who have more experience and/or knowledge. Though my study cannot evaluate this possibility, it can nevertheless speak to entrepreneurship outcomes given that, in practice, experienced investors are not the primary source of support for most new businesses (Gartner, Frid, and Alexander 2012; Ruef 2010). Rather, a substantial amount of the feedback and support that entrepreneurs receive comes from individuals in their social network, many of whom are not trained to evaluate business proposals. This is increasingly the case given the rise of web-based crowdfunding, where thousands of untrained individuals support new ventures (Mollick 2014). Additionally, despite a lack of training in hiring practices, studies find that students' ratings of employment applications are similar to managers' (Correll, Benard, and Paik 2007; Olian and Schwab 1988).

Design

In all three studies, participants rated a pair of fictitious entrepreneurs, presented as real, of the same gender, age, and level of qualifications, and whose organizations were in the same industry. Each study employed a 2×2 mixed factorial design that manipulated 1) the innovativeness of the business (innovative or non-innovative, within subjects), and 2) the gender of the entrepreneur (man or woman, between subjects). Therefore, each participant read about and evaluated one non-innovative entrepreneur and one innovative entrepreneur who were both men or women. Participants were randomly assigned to one of these conditions.

This design generates a valuable test of my hypotheses for two reasons. First, because the purpose of this project is to assess how the effect of innovation varies by gender of entrepreneur, it is important that innovation be measured as a within-subjects comparison, as it is more efficient than between-pair comparisons (Cohen 1988). Second, estimating gender effects between subjects minimizes suspicion about the study's hypotheses and produces unbiased comparisons of ratings of the same businesses across gender.

Procedure

Participants came into the lab individually and read about and evaluated descriptions of two entrepreneurs and their businesses. I counterbalanced which organization, innovative or non-innovative, they viewed first. Before leaving, they were

interviewed to assess whether the experimental manipulation was successful and to determine whether they had any suspicions about the study. Then they were debriefed and paid.

Cover Story

The studies simulated an investment scenario in order to increase task engagement and to measure bias. Participants were told that the summaries were submissions to an investment competition for young entrepreneurs that occurred four years prior. To encourage participants to put themselves in the role of what others would do, they were told that the researchers have data about each of these businesses' rates of profit and loss in the time since they launched, and that they have allocated each participant a total of 100 points (equivalent to 100 GBP or 100 USD) to "invest" in the two businesses. Participants were told they could earn £5/\$5 in returns above the £5/\$5 participation payment already promised, depending on the accuracy of their decision when compared to existing performance data.

The Descriptions

Descriptions were identical across condition, except for varying first names to manipulate gender (see below). Both entrepreneurs were described as holding undergraduate degrees from a large, upper-tier university, were the same age, had five years of management experience in the industry of their start-up, and had a credit rating that met requirements for a business loan from a major bank.

In Studies 1 and 2 (conducted in the UK and US, respectively), participants evaluated plans in a gender-neutral industry, whereas in Study 3 (US), participants evaluated plans in a high-tech industry. The gender-neutral proposals were in "the wine industry," described as an upper-middle-class, gender-neutral industry. ("Approximately 90% of owners in the industry hold at least a bachelor's degree and about 50% are women.") Both entrepreneurs held degrees in Business Management. In contrast, the high-tech proposals were both in the energy industry and proposed by individuals with degrees in Environmental Engineering.

Gender manipulation

Gender was manipulated by altering first names: Laura/Julie (women) and David/Jason (men).

Innovation manipulation

The innovation manipulation was designed to capture the theoretical dichotomy between a business model that replicates existing organizations versus one that departs from existing practices by introducing a new product or process. To make differing levels of cognitive legitimacy explicit, the non-innovative proposals were described as "common" and "shown to work in the past," whereas innovative proposals were described as "especially innovative." In the gender-neutral descriptions, the non-innovative summary described a typical wine store, whereas the innovative summary described a store that provides customers the ingredients,

tools, and guidance to make and bottle their own wine.¹ In the male-typed industry descriptions, the non-innovative entrepreneur plans to start a typical consulting firm in which “engineers and technicians would consult with clients to increase the energy efficiency of homes and businesses.” In contrast, the innovative entrepreneur has designed a new geothermal energy system that is far more efficient and cost effective than current ones and is in the process of patenting the design.²

Pretests indicated that the innovative descriptions were perceived to be significantly more innovative than the non-innovative descriptions when no information was provided about the entrepreneur. Manipulation checks during the studies also confirmed that participants rated the “innovative” plans to be significantly more innovative than the non-innovative plans ($p < 0.001$ in all three studies).³ Most participants also described the innovative plans as “innovative” and/or “risky” in an open-response item at the end of the study. Two participants were eliminated due to failed manipulation checks.

Dependent Measures

Status beliefs measures

Participants rated how competent, skilled, and committed they thought each entrepreneur was. Each item was measured on a scale ranging from 1 (“not at all”) to 5 (“extremely”). After rating each proposal, participants compared the competence of the entrepreneurs to each other. Answers ranged on a seven-point scale, with 1 indicating the entrepreneur was much less competent than the other, and 7 indicating the entrepreneur was much more competent.

Business evaluation measures

Participants began by rating how profitable and competitive each enterprise would be, the extent to which it could be made successful in the long term, and the extent to which they would be personally interested in investing in it. Each item was measured on a scale ranging from 1 (“not at all”) to 5 (“extremely”). Because these items closely map onto one another, I created a single “Business Validation” index that reflects a participant’s overall level of confidence in and support for the business idea ($\alpha = 0.75$ in Study 1, $\alpha = 0.78$ in Study 2, and $\alpha = 0.76$ in Study 3). Then, participants divided 100 “investment points” between the two businesses. This item serves as a behavioral measure of support (since participants were told that their payment depended on the accuracy of their decision) and also reflects their *relative* level of support for the innovative versus non-innovative organization.

Results

Study 1: Gender Status Beliefs and Innovation

Study 1, conducted in the UK, examines my first two hypotheses about the salience of gender status beliefs in entrepreneurship (H1 and H2). Table 1 shows means by condition. The first two columns indicate that men are penalized for

Table 1. Means for Status and Evaluation Variables by Gender and Innovativeness of Business Plan, Study 1

	Male entrepreneurs		Female entrepreneurs	
	Non-innovative	Innovative	Non-innovative	Innovative
<i>Status variables</i>				
Competence	3.86 (0.57)	3.14 (0.79)***	3.17 (0.64)	3.54 (0.51)*
Relative competence	4.05 (1.32)	3.95 (1.32)	3.04 (1.16)	4.96 (1.16)***
Skill	3.29 (0.56)	3.05 (0.92)	3.08 (0.83)	3.13 (0.74)
Commitment	3.90 (0.62)	4.10 (0.88)	3.71 (0.62)	4.00 (0.66)
<i>Evaluation variables</i>				
Business validation index	3.31 (0.67)	2.49 (0.69)***	2.84 (0.48)	2.91 (0.67)
Investment points	68.33 (19.65)	31.67 (19.65)***	49.38 (25.80)	50.63 (25.80)

Note: Standard deviations shown in parentheses.
* $p < .05$ one-tailed test for means between innovators and non-innovators; *** $p < .001$

innovation: not only are male innovative entrepreneurs rated less competent than their non-innovative counterparts ($p < 0.001$), but their businesses are deemed less worthy of support by both the business validation index ($p < 0.001$) and investment points ($p < 0.001$). This finding supports the theoretical notion that innovative entrepreneurs encounter social resistance and may even be perceived as foolhardy. However, these patterns do not hold in the female condition. Innovative women entrepreneurs are perceived to be more competent ($p < 0.05$; relative measure: $p < 0.001$) than their non-innovative counterparts, and innovation is not associated with the level of support their businesses receive.

In order to more fully evaluate my hypotheses, I turn to regression models that estimate the effects of gender, innovativeness, and the interaction between gender and innovativeness on each dependent measure. I use random intercepts regression models to take into account the nonindependence of observations that results from asking participants to evaluate entrepreneurs in pairs.

Estimated regression coefficients are presented in table 2. In most models, the gender coefficient and the interaction between gender and innovativeness are in the opposite direction, indicating that participants assign relatively low baseline ratings to women entrepreneurs, but are far less likely to penalize women for innovation.

In support of H1, the effects for Woman Entrepreneur indicate that non-innovative women entrepreneurs are rated significantly less competent than their male counterparts ($\beta = -0.69$, $p < .001$). Specifically, non-innovative women entrepreneurs are rated about 0.7 points lower on the five-point competence scale (mean for men = 3.86; mean for women = 3.17). The coefficient for the relative

Table 2. Estimated Regression Coefficients for the Effects of Gender and Innovation on Status and Business Evaluation Variables, Study 1

	Status variables				Evaluation variables	
	Competence	Relative competence	Skill	Commitment	Validation index	Investment points
Innovative entrepreneur	-0.71*** (0.17)	-0.10 (0.38)	-0.24 (0.24)	0.19 (0.21)	-0.82*** (0.19)	-36.67*** (7.14)
Woman entrepreneur	-0.69*** (0.19)	-1.01** (0.37)	-0.20 (0.22)	-0.20 (0.21)	-0.46** (0.18)	-18.96** (6.75)
Innovative × Woman entrepreneur	1.09*** (0.23)	2.01*** (0.52)	0.28 (0.32)	0.10 (0.28)	0.89*** (0.26)	37.92*** (9.78)
Intercept	3.86*** (0.14)	4.05*** (0.27)	3.29*** (0.17)	3.90*** (0.15)	3.31*** (0.13)	68.33*** (5.05)

Notes: Standard errors shown in parentheses.

** $p < .01$; *** $p < .001$

measure ($\beta = -1.01$, $p < .01$) further indicates that non-innovative women are, on average, rated less competent than their innovative female counterparts (mean = 3.04), whereas non-innovative men are rated as having about the same level of competence as their innovative male counterparts (mean = 4.05). Non-innovative women's businesses are also significantly penalized on the business evaluation variables: they are rated about half a point lower on the five-point validation index (business validation index: $\beta = -0.46$, $p < .01$) and receive about 20 fewer investment points (investment points: $\beta = -18.96$, $p < .01$) when compared to their non-innovative male counterparts.

However, the effects of organizational innovation differ considerably by gender of entrepreneur. Whereas innovative men entrepreneurs receive significantly lower competence, business validation, and investment ratings than their non-innovative male counterparts, the significant and positive innovative*woman interactions indicate that innovative women entrepreneurs do not experience such penalties. This finding supports the theory that by better fitting the agentically masculine entrepreneur stereotype, innovative women may signal additional "evidence" of entrepreneurial ability (H2b), a dynamic that buffers them from the skepticism that innovation might otherwise trigger.

Mediation analysis

To complete my argument that gender status beliefs help explain gender disparities in support for new enterprises, I need to give evidence that these disparities arise *because* gender informs the performance expectations that people hold for entrepreneurs. Specifically, if people have lower expectations for women entrepreneurs' competence, and these lower expectations prompt them to both favor men's non-innovative businesses over women's and rate women more positively when innovative ideas are considered, then evaluations of competence should mediate these gender effects. I evaluate this argument in table 3, where I include the competence measure as an independent variable in the models predicting

Table 3. Estimated Regression Coefficients for the Mediation of Competence on the Impact of Gender and Innovation on Business Evaluations, Study 1

	Validation index	Investment points
Innovative entrepreneur	-0.57 (0.19)***	-35.80 (6.27)***
Woman entrepreneur	-0.22 (0.19)	-9.77 (6.33)+
Innovative × Woman entrepreneur	0.51 (0.27)*	19.53 (9.29)*
Competence	0.35 (0.09)***	
Relative competence		9.14 (1.77)***
Intercept	1.95 (0.40)***	31.34 (8.42)***

Note: Standard errors shown in parentheses.
+ $p < .10$; * $p < .05$; *** $p < .001$

business evaluations. Because the investment point measure reflects the *relative* amount of support for each business, I use the relative competence measure to mediate this variable.

Not surprisingly, higher competence ratings predict significantly higher business quality ratings. More importantly, however, including ratings of competence in the models substantially reduces (and in most cases eliminates) the significant gender effects found in the business validation index and investment point measures. Specifically, the magnitude of the main effect for woman entrepreneur was reduced by 52 percent for business validation and 48 percent for investment points; the size of the interaction effect between gender and innovativeness was reduced by 43 percent for business validation and 48 percent for investment points.⁴ These findings suggest that participants rated women’s businesses differently from men’s largely *because* they believed women entrepreneurs were less competent than men entrepreneurs (i.e., because gender was salient as a status characteristic in this setting).

Discussion

This study examined my first two hypotheses. Findings suggest that, in a setting where gender can be expected to be quite salient as a status characteristic for entrepreneurs, the interactions through which entrepreneurs seek encouragement and support for a new business are likely influenced by gender status beliefs. Specifically, status-based performance expectations regarding competence (but not commitment) disadvantage women entrepreneurs and distort the perceived viability of an innovative plan. Thus, gender status beliefs likely play a role in determining which entrepreneurs and ideas come to be selected into the surviving organizational population.

Consistent with H2b, innovation is more positively associated with ability ratings for women than men, suggesting that women entrepreneurs may need to demonstrate more evidence of entrepreneurial ability than their male counterparts. By introducing an innovative organization, a woman entrepreneur signals a level of agency that is not expected for women in general, but that better fits the masculine stereotype of the “entrepreneur.” As a result, women are less likely than their male counterparts to be penalized for being (unexpectedly) innovative, and in doing so, end up partially compensating for the status-based biases they might otherwise experience.

Though these findings align with theoretical predictions, it is not yet clear whether these patterns would hold in a setting where the scope condition of entrepreneurship as a male-typed task is relatively less valid. Study 2 addresses this question.

Study 2: Comparing Study Settings

Study 2 evaluates my third hypothesis that the salience of gender status beliefs will vary across settings in which the aggregate gender composition of entrepreneurs and managers differ. This study is identical to Study 1, but was conducted at a US university.

Table 4 compares means by condition for all dependent measures. In contrast to Study 1, male entrepreneurs are not penalized for innovation. In fact, innovation confers some social (though not financial) rewards, given that innovative men are rated more competent ($p < .01$) and committed ($p < .01$) than their

Table 4. Means for Status and Evaluation Variables by Gender and Innovativeness of Business Plan, Study 2

	Male entrepreneurs		Female entrepreneurs	
	Non-innovative	Innovative	Non-innovative	Innovative
<i>Status variables</i>				
Competence	3.68 (0.72)	3.93 (0.77)	3.91 (0.69)	4.22 (0.71)*
Relative competence	3.46 (1.50)	4.54 (1.50)**	3.53 (1.50)	4.47 (1.50)**
Skill	3.57 (0.74)	3.68 (0.47)	3.25 (0.57)	3.72 (0.68)**
Commitment	3.68 (0.72)	4.21 (0.68)**	3.66 (0.65)	4.28 (0.63)***
<i>Evaluation variables</i>				
Business validation index	3.07 (0.10)	2.94 (0.13)	3.04 (0.72)	2.95 (0.75)
Investment points	53.57 (24.72)	46.43 (24.72)	46.41 (26.28)	53.59 (26.28)

Note: Standard deviations shown in parentheses.

* $p < .05$ one-tailed test for means between innovators and non-innovators; ** $p < .01$; *** $p < .001$

non-innovative counterparts. However, innovation is again more positively associated with status measures for women than for men: innovative women entrepreneurs are rated significantly more competent ($p < .05$ and $p < .01$ for both measures, respectively), skilled ($p < .01$), and committed ($p < .001$) than their non-innovative female counterpart.

Table 5 presents regression estimates for each dependent variable for Study 2 and includes significance tests for differences between coefficients for Study 1 and Study 2, which were obtained through a pooled model that included a Study 1 dummy variable, as well as the two-way and three-way interactions between Study 1, innovation and gender (not shown).

Consistent with H3, the gender effects in Study 2 follow the same pattern as Study 1, but are smaller in magnitude than in Study 1. For example, similar to the competence ratings in Study 1, non-innovative women entrepreneurs are rated significantly less skilled than their male counterparts ($\beta = -0.32, p < .05$) (a penalty of about a third of a point on a five-point scale), but this bias disappears when women present an innovative idea ($\beta = 0.36, p < .05$). The modestly significant interaction effect between Innovative and Woman Entrepreneur also indicates that the allocation of investment points is reversed for men and women: whereas innovative men received relatively fewer investment points than their non-innovative counterparts, innovative women received more ($\beta = 14.33, p < .10$). In fact, innovative women entrepreneurs receive approximately the same amount of investment points as non-innovative men entrepreneurs. Unlike Study 1, however, competence and business validation ratings do not differ significantly by gender.

US participants also held higher performance expectations for innovative men and women entrepreneurs, rating them more competent ($\beta = 0.25, p < .05$; relative measure: $\beta = 1.07, p < .01$) and committed ($\beta = 0.52, p < .001$) than their non-innovative counterparts.

Table 5. Estimated Regression Coefficients for the Effects of Gender and Innovation on Status and Business Evaluation Variables, Study 2

	Status variables				Evaluation variables	
	Competence	Relative competence	Skill	Commitment	Validation index	Investment points
Innovative entrepreneur	0.25* ^{†††} (0.15)	1.07** [†] (0.40)	0.11 (0.14)	0.52*** (0.15)	-0.13 ^{††} (0.18)	-7.14 ^{††} (6.83)
Woman entrepreneur	0.23 ^{†††} (0.19)	0.07 [†] (0.39)	-0.32* (0.16)	-0.02 (0.17)	-0.03 [†] (0.18)	-7.17 (6.62)
Innovative x Woman entrepreneur	0.06 ^{†††} (0.20)	-0.13 ^{††} (0.55)	0.36* (0.19)	0.09 (0.21)	0.03 ^{††} (0.25)	14.33 ^{††} (9.96)
Intercept	3.68*** (0.14)	3.46*** (0.28)	3.57*** (0.12)	3.68*** (0.13)	3.07*** (0.13)	53.57*** (4.83)

Note: Standard errors shown in parentheses.
* $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$; [†]Coefficients differ significantly from Study 1 at $p < .05$;
^{††} $p < .01$; ^{†††} $p < .001$

Discussion

Study 2 offered a more conservative test of my hypotheses than Study 1 because gender could be expected to be less salient as a status characteristic for entrepreneurs in a US context. And indeed, results showed only modest support for my hypotheses. Participants held lower baseline expectations for women entrepreneurs' skills (but not competence or commitment) (H1), and women entrepreneurs appeared to be more skilled when they presented an innovative idea (H2b). Moreover, resistance to investing in innovative ideas was moderately weaker for women than men entrepreneurs (H2b).

The weaker gender effects in the US versus UK setting (H3) suggest that the relevance of gender status beliefs at the micro level may be at least partly conditional upon patterns of inequality at the macro level. Yet, bias in entrepreneurial ability was detected with the skill measure in the US setting, but with the competence measure in the UK setting. Although this discrepancy was unexpected, it is possible that "competence" may be interpreted as a general indicator of ability, whereas "skill" implies a level of specific know-how that may be learned. If so, UK respondents may be more likely to view women as generally less capable of entrepreneurship, whereas US respondents may be more likely to view women as less prepared for entrepreneurship. This interpretation is consistent with the finding that in the UK setting, participants produced substantially biased ratings of competence as well as business viability, whereas participants in the US setting produced biased evaluations of women's skills, but less biased ratings of business viability.

In the US setting, participants also associated innovation with greater ability and effort, and innovative men experienced smaller penalties on the business validation and investment measures than in the UK. This finding suggests that there is generally greater status and less skepticism associated with innovation in the United States, which is not surprising in light of the uniquely strong tradition of entrepreneurship and innovation in American culture (Schumpeter 1961[1934]; Shane 1993; Weber 1930[1904]).

One limitation of these two studies is that findings could be an artifact of the particular vignettes used. For example, findings could have been influenced by unobserved cultural or gendered associations that respondents made with the wine industry, over and above the gender-neutral information that was provided. Moreover, as theorized above, the salience of gender status beliefs in entrepreneurship likely varies according to industry context. Study 3 addresses these issues.

Study 3: Industry Effects

Study 3, conducted at the same US university as Study 2, tests my fourth hypothesis that findings will differ when the industry of a start-up is male dominated and requires male-typed skills. Accordingly, the design for Study 3 is identical to Studies 1 and 2, but the business descriptions are in a high-tech industry.

Table 6 shows means for Study 3. Similar to Study 2, innovative male entrepreneurs are perceived to be relatively more competent ($p < .05$) and committed

Table 6. Means for Status and Evaluation Variables by Gender and Innovativeness of Business Plan, Study 3

	Male entrepreneurs		Female entrepreneurs	
	Non-innovative	Innovative	Non-innovative	Innovative
<i>Status variables</i>				
Competence	3.88 (0.55)	4.03 (0.47)	3.73 (0.59)	4.02 (0.57)*
Relative competence	3.66 (1.26)	4.34 (1.26)*	3.88 (1.33)	4.12 (1.33)
Skill	3.81 (0.64)	3.88 (0.66)	3.46 (0.67)	3.82 (0.67)**
Commitment	3.81 (0.59)	4.25 (0.67)**	3.66 (0.85)	4.12 (0.75)**
<i>Evaluation variables</i>				
Business validation index	3.23 (0.49)	3.20 (0.11)	3.14 (0.65)	3.49 (0.69)*
Investment points	48.34 (3.84)	51.53 (3.86)	46.90 (3.63)	52.85 (3.65)

Note: Standard deviations shown in parentheses.
 * $p < .05$ one-tailed test for means between innovators and non-innovators; ** $p < .01$

($p < .01$) than their non-innovative counterparts, and they do not experience penalties in the evaluations of their businesses. Innovative women entrepreneurs also receive higher competence ($p < .05$), skill ($p < .01$), and commitment ($p < .01$) ratings than their non-innovative counterparts. Yet, unlike the other studies, participants in this setting rate innovative women’s businesses *more* worthy of support than non-innovative women’s businesses ($p < .05$), a reward of about a third of a point on the five-point business validation index.

Table 7 presents regression estimates for each dependent variable. Tests for significant differences between Study 3 and Study 2 coefficients were obtained from pooled models that included a Study 3 dummy variable, as well as the two-way and three-way interactions between Study 2, innovativeness and gender (not shown).

The overall patterns of effects in table 7 are similar to the previous studies. For instance, the interaction effect between Innovative and Woman Entrepreneur on the business validation index is significant and positive ($\beta = 0.54, p < .05$), indicating that innovation is associated with more favorable perceptions of business potential for women entrepreneurs than men. This finding parallels both business evaluation measures in Study 1 and the investment point measure in Study 2. Also, like Study 2, gender bias emerges in ratings of entrepreneurial skill: participants rate non-innovative high-tech women entrepreneurs to be significantly less skilled than their male counterparts ($\beta = -0.32, p < .05$), though this bias diminishes when women entrepreneurs demonstrate innovativeness ($\beta = 0.26, p < .10$).

Despite these similarities, there are a few key differences between the high-tech and the gender-neutral settings, showing modest evidence for the prediction that

Table 7. Estimated Regression Coefficients for the Effects of Gender and Innovation on Status and Business Evaluation Variables, Study 3

	Status variables				Evaluation variables	
	Competence	Relative competence	Skill	Commitment	Validation index	Investment points
Innovative entrepreneur	0.20* (0.10)	0.65* (0.32)	0.09 (0.13)	0.47*** (0.14)	0.04 (0.15)	3.76 (5.50)
Woman entrepreneur	-0.11 ^b (0.13)	0.19 (0.29)	-0.32* (0.15)	-0.12 (0.17)	-0.03 (0.14)	-0.93 (5.21)
Innovative × Woman entrepreneur	0.07 (0.13)	-0.40 (0.42)	0.26+ (0.18)	-0.04 (0.19)	0.26+ ^a (0.19)	1.79 (7.34)
Intercept	3.85*** (0.10)	3.67*** (0.23)	3.80*** (0.12)	3.80*** (0.13)	3.19*** (0.11)	48.06*** (3.95)

Note: Standard errors shown in parentheses.

+ $p < .10$; * $p < .05$; *** $p < .001$

^a Coefficients differ significantly from Study 2 at $p < .10$; ^b $p < .05$

gender effects would be larger in a high-tech industry (H4). In Study 3, women entrepreneurs received somewhat lower competence ratings than men, whereas this was not the case in Study 2 (coefficients for “Woman entrepreneur” are significantly different at the $p < .05$ level). These somewhat lower baseline expectations for women are consistent with the finding that the positive interaction effect between gender and innovativeness on investment is modestly larger in a male-dominated, high-tech industry than in a gender-neutral industry ($p < .10$).

General Discussion

Both classical and contemporary theorists of organizations and entrepreneurship have posited that cultural beliefs matter in the formation of new and novel organizations. By drawing on social psychological theory, this article is the first to specify and empirically test how certain cultural beliefs about gender may frame the social interactions that ultimately determine whether a new organization will survive. Findings from three experimental studies across two cultural contexts indicate that gender status beliefs play a key role in determining the likelihood that a new organization will be deemed worthy of support.

First, across all three studies, participants held lower expectations for women entrepreneurs’ abilities and the viability of their business plans than for men entrepreneurs’ *in general* (i.e., for non-innovative entrepreneurs). This finding underscores the theoretical notion that gender status beliefs—specifically as they pertain to entrepreneurial ability rather than effort—are a plausible mechanism that fuels gender biases in entrepreneurship. Second, innovation was more strongly and positively associated with performance expectations for women than men. This finding supports the theoretical proposition that innovation can signal additional evidence of entrepreneurial ability for women: rather than exacerbating disadvantage (H2a), innovation mitigates gender bias by counteracting,

to some extent, lower expectations for women's abilities in entrepreneurship (H2b). In short, women entrepreneurs had less to lose and more to gain by introducing an innovative business model; by doing so, they signaled personal qualities that better fit with the agentically masculine stereotype of the entrepreneur.

In contrast, the effects of innovation on evaluations of men entrepreneurs' abilities and ideas were less consistent across the studies. In the UK study, innovative men were rated less competent and worthy of support than their non-innovative counterparts. In the US studies, innovative men were rated more competent and committed, but not any more or less worthy of support than their non-innovative counterparts. These patterns suggest that when organizational innovations are introduced by men, they may be more subject to cultural variability in attitudes toward innovation and risk-taking. This is because, for men, innovation does not simultaneously signal evidence of a particular ability they are generally thought to lack. In effect, men's innovations appear to be judged more on their perceived legitimacy (or lack thereof), whereas women's innovations appear to be judged as partial compensation for their perceived lack of entrepreneurial ability.

Third, the pattern of gender bias was similar across study settings, but effects were larger in the settings where entrepreneurship was more male typed, and thus where gender could be expected to be more salient as a status characteristic: the UK, where men's aggregate representation in entrepreneurship and business leadership roles in general is higher, and a high-tech industry, where men's representation is also higher. The US/UK comparison in particular highlights how the basic content of gender stereotypes—such as women's presumed lack of competence or agency in a male-typed domain—is similar across these societal contexts, but that the relative impact of such stereotypes on individuals' propensity to discriminate may be conditional upon the extent of men's overrepresentation in a given male-typed domain. Furthermore, as the comparisons in effect sizes between studies indicate, the differences in findings between societal contexts were also substantially larger than the differences in findings between industries. This pattern suggests that gender status beliefs about entrepreneurial activity *in general* likely carry greater responsibility for gender bias in entrepreneurship than do status beliefs about industry-specific skills.

Because social interactions are complicated by a multitude of factors, it is difficult to use observational techniques to systematically assess status-driven biases. In this regard, testing the theory in a controlled laboratory setting was advantageous. This method also avoids sampling on the dependent variable (e.g., interviewing successful entrepreneurs). Experiments are limited, however, in that they cannot address the extent to which gender status beliefs influence the interactions of actual entrepreneurs. Thus, although I can evaluate status beliefs as one possible mechanism responsible for patterns of inequality in entrepreneurship, I cannot assess the relative importance of this mechanism vis-à-vis other factors.

Additionally, although the pattern of gender and innovativeness effects is consistent across all three studies, my study design does not allow me to evaluate the possibility that participants rated innovative women's ability higher than non-innovative women's in order to compensate for biasing against non-innovative women. Some research has shown that individuals are more likely to express

prejudiced viewpoints when they also have the opportunity to demonstrate non-prejudicial attitudes (Monin and Miller 2001). By making this compensation, individuals retain their “moral credentials.” Because participants directly compared non-innovative women to innovative women, they may have unconsciously embellished their ratings of the innovative entrepreneur to make up for low ratings of the non-innovative entrepreneur.

Finally, whereas this study examined the effect of organizational innovations within existing industry categories, it is possible that participants would have penalized innovative organizations more strongly had they introduced something that is more unfamiliar and unrelated to existing products and services. Participants may have also reacted differently had the innovations centered on novel processes, such as methods of production or supply chains. Investigating how differing degrees and forms of innovation trigger differing levels of skepticism and bias would be a key question for future research.

Theoretical Contributions and Next Steps

This research makes important contributions to the areas of gender, organizations, and social psychology. To begin, this study develops status beliefs as a “demand-side” mechanism for understanding women’s underrepresentation in entrepreneurship, a form of gender inequality that has typically been understood through a “supply-side” lens. I show that, despite being less constrained by pre-existing organizational roles, gender status beliefs are salient in the context of entrepreneurship and are responsible, at least in part, for the disadvantages that women entrepreneurs are known to experience. Status beliefs bias the perceived viability of new organizations, producing larger disadvantages for women entrepreneurs in societal and industry contexts where their representation among entrepreneurs at the aggregate level is lower. This finding indicates that the long-held theoretical claim—that the gender composition of a woman’s occupation, workplace, and/or job matters for the way she is perceived and evaluated in day-to-day interactions (e.g., Kanter 1977)—also applies in the entrepreneurship domain.

The implication of these findings is that gender status beliefs likely disadvantage most women entrepreneurs, given that 1) most entrepreneurs (especially women) do not start businesses that are particularly innovative (Ruef 2010; Tonoyan and Strohmeier 2005), and 2) women are vastly underrepresented among entrepreneurs in most societies (Kelley, Bosma, and Amorós 2010). If status beliefs lead most people to doubt women’s entrepreneurial ability, even subtly, women may be discouraged from pursuing entrepreneurship in the first place, less likely to persist in an entrepreneurial career, and/or disadvantaged when they seek support for their venture. Although these findings suggest that women may be able to strategically mitigate their vulnerability to bias by being innovative, this strategy would not fundamentally challenge gender beliefs given that they are conditioned on macro-level inequality. Therefore, the problem of gender inequality in entrepreneurship should be understood as a problem that is rooted in the interrelated social and economic institutions that lend support to gender inequality in the labor market more broadly.

This study also introduces a new method for evaluating how forms of gender inequality are reproduced in modern societies. In particular, it is the first to employ a controlled experiment across two cultural contexts with the goal of identifying how gender status beliefs about women's abilities in a particular domain play out differently when there are differing levels of gender inequality in that domain at the macro level. By doing so, this study contributes to multilevel theories of gender, which posit that distributions of resources at the macro level sustain gender inequality in part through their influence on micro-level social interactions (Ridgeway and Correll 2004; Risman 1998).

Next, my findings contribute to organization theory by suggesting that the emergence of novel organizations can be understood to be, at least in part, a function of status beliefs. This finding not only offers a micro-level mechanism for understanding why some organizations survive whereas others fail, but it also integrates social psychological perspectives on status processes with cultural-cognitive institutional approaches (e.g., DiMaggio and Powell 1991). Whereas experimental methods have been broadly applied to address the formation and legitimation of status beliefs, this is the first study to use them to investigate how cultural-cognitive processes affect perceptions of new organizational forms.

By linking the macro-social and organizational context to micro-level cognitive processes, I also underscore multilevel theories of entrepreneurship (Ruef and Lounsbury 2007) and organizational theorists' understanding of cognitive legitimacy (Suchman 1995). For instance, while cognitive legitimacy is often understood to be contingent on macro-level conditions, such as the size of an industry, my work suggests that perceptions of cognitive legitimacy may be affected by status beliefs at the micro level. Therefore, organizational populations that come to be taken for granted should be understood as being shaped in part by status beliefs. This is important given that the characteristics of individual founders play a role in determining the types of organizational structures and practices that firms adopt (Baron, Hannan, and Burton 1999).

Finally, this work extends social psychological research on status beliefs to the context of nascent entrepreneurship. This approach contrasts with most previous work, which has focused on how status-based performance expectations operate in task-oriented small groups or employment settings.

One important avenue of future work will be to examine the extent to which gendered patterns of feedback persist in field settings and among different samples of study participants, especially those involved in providing formal feedback to entrepreneurs, such as investors or educators. Such samples would help provide a broader picture of the extent to which gender status beliefs actually affect the day-to-day experiences of entrepreneurs.

It would also be productive to examine in greater detail the different ability standards to which men and women entrepreneurs appear to be held. For instance, aside from demonstrating greater ingenuity, women entrepreneurs may also need more human or social capital to convince stakeholders that their businesses are equally worthy of support. Indeed, a recent study suggests that technical knowledge and social ties may be more beneficial for women than men in high-tech entrepreneurship (Tinkler et al. 2015). Another important step would be to broaden the scope conditions of my account. For instance, Yang and Aldrich

(2014) find that women need to demonstrate more consistent evidence of merit before they take the lead on entrepreneurial teams, a dynamic that may be fueled by gender status beliefs. Status beliefs may also affect the degree to which individuals are able to garner support for innovative ventures *within* established organizational contexts.

Finally, although I evaluated gender as one relevant status characteristic in the context of entrepreneurship, similar processes may occur along the lines of other status characteristics, such as age, nationality, class, and race/ethnicity. Thus, status beliefs may be one common lens for understanding the micro-level processes that underpin macro-level patterns of stratification in entrepreneurship.

Notes

1. The “innovative” idea is based on a small Southern California business that won awards for innovation from its chamber of commerce.
2. This description is adapted from the winner of an investment competition for undergraduate entrepreneurs at Princeton University.
3. These perceptions did not vary by gender of entrepreneur. Additionally, innovativeness ratings of the wine vignettes did not vary significantly by study setting.
4. Alternative models that further included interactions between competence and innovation produced similar results. These interactions are not statistically significant.

About the Author

Sarah Thébaud is Assistant Professor of Sociology at the University of California–Santa Barbara and a faculty research associate of the Broom Center for Demography. Her research investigates social psychological and macro-institutional sources of gender inequality in the new economy. In addition to research on gender stratification in entrepreneurial activity, her recent publications and current projects examine the sources of persistent gender biases and inequalities in families, work, investment markets, science, and higher education.

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Social Belonging and Economic Action: Affection-Based Social Circles in the Creation of Private Entrepreneurship

Dali Ma, *Drexel University*

Most social network studies following Granovetter's (1985) vision of embeddedness have either focused on instrumental relations or lumped instrumentality and sentimentality together. This study seeks to clarify whether social relations that primarily build on sentimentality can impact economic action. Based on the context of Chinese market transition, this paper found that general managers that had affection-based social circles, that is, small groups in which people enjoy being together, were more likely to start a private firm after being laid off. In contrast, business-based social circles, defined as small groups mainly formed on business interests, did not have a significant interactive effect with layoff. These findings are consistent with the argument that affection-based social circles help managers experiencing job loss maintain a stable and positive self-identity, and that these circles also exert less constraint over radical career change.

Society and conversation, therefore, are the most powerful remedies for restoring the mind to its tranquility, if, at any time, it has unfortunately lost it; as well as the best preservatives of that equal and happy temper, which is so necessary to self-satisfaction and enjoyment.

—Adam Smith, *The Theory of Moral Sentiments* (1759)

Granovetter (1985) questioned both the atomic and over-socialized views of economic behavior, suggesting that economic action is embedded in concrete ongoing social relations. Stimulated by his insight, social network analysis has become arguably the most important theoretical angle in economic sociology (e.g., Baker 1984; Bian 1997; Burt 1992; Uzzi 1996). Nonetheless, most studies following Granovetter's (1985) vision have either focused on instrumental relations or lumped instrumentality and sentimentality together, failing to elucidate whether

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social relations that are primarily based on sentimentality can impact economic action.

This study analyzes the role of affection-based social circles in helping laid-off managers start a private business. Affection-based social circles refer to small groups in which people enjoy being together (e.g., a group of school buddies), and business-based social circles refer to small groups primarily built on business interests (e.g., a group of close business partners). Whereas job loss jeopardizes managers' professional identity as well as their personal identity in business-based social circles, affection-based social circles transcend career turbulence, letting the victims maintain a stable and positive self-identity for transitioning into the distinctive role of private entrepreneur.

With regard to entrepreneurship research, although social networks analysis is key to the sociology of entrepreneurship (Ruef and Lounsbury 2007; Stuart and Sorenson 2005), existing entrepreneurship literature typically applies social network theories rather than addressing the ways that entrepreneurship studies contribute to social network analysis. This paper joins a handful of studies (Ruef 2010; Portes and Sensenbrenner 1993) to demonstrate entrepreneurship research's contribution to a core topic in social network analysis—embeddedness.

Embeddedness

Although Granovetter's (1985) idea of embeddedness inspired numerous studies in economic sociology, most research on embeddedness has focused on either instrumental relations or relations that mix instrumentality and sentimentality, thereby obscuring whether social relations primarily building on sentimentality can influence economic action. A major proponent of the instrumental view of social networks, Burt (1992), stresses "strategic embedding" where a broker bridges previously unconnected players to gain information and control benefits, and he claims that the "sense of investing in people with whom you think good things could happen before you are sure what these things are captures the essence of brokerage" (Burt 2007, 95). However, it is difficult, and sometimes impossible, to envision instrumental benefits of social interactions. People attending a party for fun could run into folks that have non-redundant job information, so it is "profoundly misleading to think of the acquisition of such information as the result of 'investment' in contacts" (Granovetter 2002, 37).

Sociologists (Portes and Sensenbrenner 1993; Podolny and Baron 1997; Small 2009) have been concerned about the instrumental view of social networks because this view implies that rational actors gain advantages by manipulating social relations. To address the concern, economic sociologists underscored the importance of business friendship, which mixes instrumentality and sentimentality. Business friends cannot escape the constraint of social relations; in fact, it is the relational constraint that helps them build trust, mitigate competition, and obtain fine-grained information (Ingram and Roberts 2000; Ruef 2010; Uzzi 1996).

Nonetheless, because economic exchange requires instrumental considerations and genuine friendship keeps instrumentality at bay, business friendship could run into an inherent tension between instrumentality and sentimentality (Ingram and Zou 2008; Silver 1990). Social action based on business friendship is instrumentally

rational (Weber 1978) because business friends fundamentally strive for attaining rationally calculated ends; in contrast, expressive friendship satisfies human emotional needs (Wolf 1966), and depicts affectual social action (Weber 1978). The distinction between these two types of friendship illuminates the difference between instrumental action for the purpose of achieving specific goals and expressive action for their own sake (Lin, Woelfel, and Light 1985). Expressive friendship is also a type of communal relationship in which people have a general obligation to be concerned about each other's welfare, but people do not have such an obligation in instrumental relationship (Clark, Mills, and Powell 1986). Expressive friendship produces ingrained social solidarity, which integrates trust, affection, unity, and extensive obligations (Hechter 1987; Molm 2010).

An ethnography illustrates that people protect communal relations from being contaminated by commercial ones. Granovetter (1995, 131) noted that in a Javanese town, "commercial relations typically did not overlap with those of kinship or neighborhood but were almost purely economic in nature." If economic advantages of combining commercial relations with social obligations are so obvious, why did the Javanese not mix them? The author of the ethnography (Dewey 1962) observed the following:

Because of the common culture and even the kin ties which Javanese traders share with rural people they can leave trading. They can retreat to a village if unpleasant sanctions are brought to bear on them by the business community. In fact unstable market conditions, slack periods, and bad luck cause even skilled and respected traders to leave trading for longer or shorter periods. There is constant movement into and out of market trade ... (186).

The Javanese traders separated economic sphere from social space so that strong social belonging embedded in communal relations could buffer economic risks. Kin, neighbors, and friends in the Javanese community are primary groups, an intimate fellowship fusing individualities into a common identity such that one lives in the feeling of the whole (Cooley 1962). As Coser (1977, 308) commented, "In other forms of association...men may be related to one another because each derives a private benefit from that interchange or interaction. In such groups, the other may be valued only extrinsically as a source of benefits for the self; by contrast the bond in the primary group is based on an intrinsic valuation of the other as a person, and appreciation of others does not result from anticipation of specific benefits that he or she may be able to offer." A primary group of friends love and wish each other well as ends in themselves, not instrumental value of each to the other (Badhwar 1987). When friends are ends in themselves, they gain profound psychological visibility, seeing themselves as a unique human being that is deeply admired and cared for (Branden 1993).

Affection-Based Social Circles

A social circle refers to three or more friends who are close to one another. I focus on triadic strong ties because a dyad "does not attain that super-personal life which the individual feels to be independent of himself" (Simmel 1964, 123) and weak ties may be too ephemeral to generate distinct groupings. Based on the distinction between affectual action and instrumentally rational action (Weber 1978), this study defines

two types of social circles, that is, affection-based social circle and business-based social circle. While people enjoy togetherness in an affection-based social circle, the pursuit of economic benefits is the main goal of a business-based social circle.

The key distinction between affection-based and business-based social circles is whether people intentionally form and maintain the circle for instrumental self-interest. Members of affection-based social circles might find resources to help a member out of life crisis, but their intention is to help the friend cope with distress instead of garnering benefits for themselves. Social exchange between members in affection-based social circles is primarily reciprocal exchange in which actors perform individual acts that benefit another without negotiation and without knowing whether or when the other will reciprocate (Molm 2010). It is more difficult to manipulate social relationships for business interests in an affection-based social circle because the norm of enjoying togetherness restrains instrumental initiatives.

Because affection-based social circles do not rely on economic utility, a person can have a stable perception that social support is available from other members when needed; the perceived social support could be an even more important predictor of positive psychological outcomes than are the specific support behaviors provided by their partners (Cunningham and Barbee 2000). Although a minimal element of affect is an important ingredient in business friendship, the charge of affect is a device for keeping the underlying economic exchange (Wolf 1966). Whereas a minimal sense of belonging could arise from shared social activities in business-based circles, the predominant instrumental orientation makes it difficult for such circles to be a source of stable social belonging because economic utility of their members may change over time.

Affection-Based Social Circles in the Creation of Entrepreneurship

This study investigates the interactive effect of layoff and affection-based social circles on private entrepreneurship. Sørensen (2007) suggests that people in bureaucratic organizations are less likely to engage in entrepreneurship because their employees are used to routines, undertake a narrow range of tasks, and lack exposure to external opportunities and resources. Additionally, job stability and internal routes of advancement increase the opportunity costs of leaving paid employment. General managers may overcome some of these constraints because their responsibility and external exposure are much broader than an average employee, but job security still makes it difficult for them to escape the bureaucratic “iron cage.” The notion of “blocked mobility” in entrepreneurship research (Saxenian 2001) implies that adverse career events such as layoff could disrupt bureaucratic inertia and reduce the opportunity costs of career change. Furthermore, general managers could develop the identity of being the “boss” through daily management. When layoff shatters this identity, general managers may pursue private entrepreneurship to obtain a sense of mastery (Thoits 1995), recover the dented identity (Ashforth 2001), and prove themselves (Schumpeter 1934).

On the other hand, job loss also leads to the loss of self-esteem and the erosion of the sense of mastery (Pearlin et al. 1981; Price, Friedland, and Vinokur 1998), which

can precipitate psychological distress (Ashforth 2001) and hinder entrepreneurial initiatives. It is at such a distressful moment that affection-based social circles could demonstrate their role in redirecting the victim's career life. Although a few studies (Brüderl and Preisendörfer 1998; Ruef 2010) have noted the importance of social support for entrepreneurship, they did not explore different types of friendship in providing social support during the receiver's career crisis. More generally, social support researchers have called for distinguishing different types of networks in providing social support (House, Umberson, and Landis 1988; Thoits 1982). Furthermore, most studies of social support have only shown that "individuals are shaped by, rather than actively construct, their social worlds" (Thoits 2003, 182). This study takes a step to bridge this gap, suggesting that social forces that are largely independent of instrumental motives help distressful economic actors take one of the most agentic economic actions—entrepreneurship.

Identity Support

When job loss suddenly ruptures general managers' professional identity, they enter a liminal stage, a roleless vacuum composed of anxiety and disorientation (van Gennep 1960). In the meantime, their identity in business-based social circles is likely to melt because the identity primarily builds on career achievement. Since one's self-feeling reflects the ideas about himself that he attributes to other minds (Cooley 1964), even subtle changes of the attitude of other people in a business-based circle may create "dual status anxiety," that is, anxiety over losing status in both the formal organization and an informal group. The anxiety could arise even without communicating with other members of the business-based circle because the victim may imagine a tainted self and worry about social exclusion.

In contrast, an affection-based social circle is formed for enjoying togetherness such that the laid-off managers can be confident that they will not be discarded by the circle and their identity associated with the circle will not be dented by career turbulence. The feelings of belonging and solidarity protect their self-esteem after adverse career events (Cohen and McKay 1984). Sometimes friends in affection-based social circles do not even need to explicitly comfort the victim because when they communicate with each other as usual, the victim could feel the continuity of social life and the sense of stability helps neutralize the turbulence precipitated by job loss.

Friends in affection-based social circles are like a backstage audience (Goffman 1959), in front of whom the performer can take off the mask, show the "true self," and release the pressure resulting from adverse career events. Because affection-based social circles are formed for the sake of interaction itself, people are more likely to join and stay in such circles where friends see each other positively (Swann, Stein-Seroussi, and Giesler 1992); the self-verification could help buffer distress during periods of disruption and change (Cast and Burke 2002). While business-based social circles may merely touch at the surface of each other's social lives, affection-based social circles extend to the level of involvement and concern that aid job losers to resist the adoption of a diminished view of themselves (Pearlin et al. 1981). Once the victim's mind is pacified, he/she can be more focused and engaging with respect to entrepreneurial endeavor.

Affection-based social circles thus provide a stable and positive self-identity that helps laid-off general managers make the transition into private entrepreneurship.

Less Career Constraint

Affection-based social circles also exert less career constraint over general managers who lose their job. Although entrepreneurship appears to deviate from their prior career path and the initial thought of creating a private business may sound premature, affection-based social circles tolerate deviation, allowing friends to share immature and radical thoughts (Suttles 2011). Mere chatter among such a group of friends not only relays amiability and cordiality, but also transcends reality (Simmel 1964), relaxing the laid-off friend who faces the challenging task of startup.

By contrast, business-based social circles are domain specific (Small 2009), mainly related to the job that the manager has lost. As noted above, the job loser may be reluctant to approach members in a business-based social circle because of the concern about a tainted self-image. Even if the victim approaches them, he/she may receive lukewarm feedback because the economic advantages associated with a private venture are far more uncertain than what instrumental friends can gain from the victim’s prior affiliation with an established organization. After the victim loses the key resource for economic exchange, business-based circles are no longer a reliable source of social support.

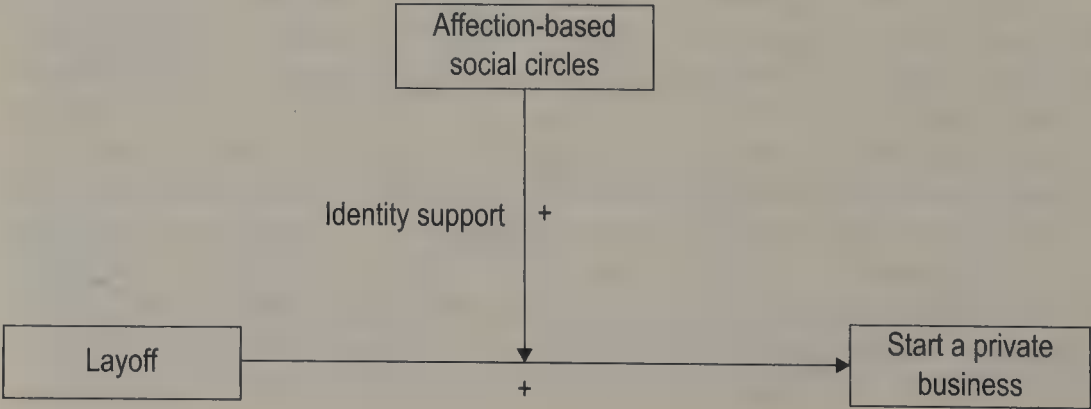
Figure 1 summarizes the main theoretical model of this study.

Context

Transitional China

The context of this study is transitional China in the 1990s. Granovetter (2002) noted that the connection between embeddedness and institutional contexts has been tenuous, which is unfortunate because social networks cannot be separated from larger social processes. As Western countries evolved into more static societies, dramatic social changes in former socialist countries have fascinated sociologists to revitalize social theory (Nee and Matthews 1996). Several studies

Figure 1. A summary of the main theoretical model



reported massive layoffs in the Chinese state sector in the 1990s (Bian 2002; Gu 1999). Layoffs by state organizations were particularly distressful because many Chinese had seen themselves as permanently belonging to state organizations (Shenkar 1996). A notable outcome of the massive layoffs was private entrepreneurship eased by the enhanced social status of private entrepreneurs (Dickson 2003; Gu 1999; Ma and Parish 2006). The Chinese context provides a good opportunity for examining whether affection-based social circles can help distressful actors start private businesses.

China is a relational society where interpersonal relationships are an important source of self-identity (Hwang 2000), and the Chinese seek security and inalienability from primary groups (Hsu 1963). Although *guanxi* has instrumental, etiquette, moral, and emotional dimensions (Guo 2001), the indigenous *guanxi* research shares the problem with embeddedness studies by underlining that the Chinese manipulate social relations to obtain instrumental benefits (Bian 1997; Wank 1999). Whereas most contemporary *guanxi* studies build on Yang's (1994) classic work, they neglect the distinction that she drew between "heart-to-heart" and instrumental friends. "One cannot use them ["heart-to-heart" friends] for gain, that is, as an instrument to acquire resources, because in real friendship, one gives without thinking of a return" (Yang 1994, 117). Yang (1994) also noted that instrumental friends could be intimate during good times, but may not be reliable during hard times. Her insight echoes a Chinese maxim—"When life is peaceful and without trouble, it is difficult to distinguish the true from the false friend. Only when difficulties arise do the true feelings of a friend reveal themselves. For in a time of crisis, true friends will draw closer, and false friends will become increasingly scarce" (Ricci 2009, 91).

Chinese Academy of Sciences

I interviewed general managers of state-owned enterprises of the Chinese Academy of Sciences (CAS), the highest-level national research institution. CAS differs from the National Academy of Sciences in the United States because it had not only an honorable membership section, but also full-time scientists; in 2000, it had 39,000 full-time scientific personnel in 108 scientific institutes around China. When Chinese economic reform began in 1978, national policies stated that science and technology should play a central role in modernization. However, the state soon realized that much research had no impact on economic development, so it pushed CAS to initiate a reform called "One Academy, Two Systems" in 1988, in which CAS established state-owned enterprises and assigned scientists and administrative staff to be general managers.

In the mid-1990s, the booming Chinese economy greatly enhanced the financial capabilities of the state, which wanted science and technology to lead economic growth. CAS proposed a reform called "Knowledge Innovation" in 1997 to build itself into a cutting-edge research center (Suttmeier, Cao, and Simon 2006). Fascinated by the proposal, the state considerably increased the allocated funds for CAS. Money was not a problem anymore; CAS wanted Nobel laureates. The reform required CAS institutes to change their state-owned enterprises into limited liability companies or shut them down. Because "One Academy, Two

Systems” had stirred identity conflict within CAS (“Are we an academy or a company?”), many institutes chose to disband their enterprises. As a result, a number of general managers of the discontinued enterprises were laid off.

Because there was a strong sense of belonging among people working in CAS, the job losers experienced considerable distress when layoff suddenly cut off the attachment. Since the “face” of the Chinese is based on their social position, being laid off by the prestigious CAS could considerably injure one’s self-esteem (Redding 1990). A number of respondents recalled mental and physical illnesses after being laid off. One of them (interviewee #72) said, “It was such a good business—why did they [the superiors at his institute] shut it down without hesitation? I didn’t know what to do next and was ill for two months.”

Data

The population of this study was general managers of CAS-owned enterprises in Beijing and Shanghai, the two most important cities for CAS’s business activities during “One Academy, Two Systems.” According to the *Registry of CAS Firms* (1999), about half of CAS-owned enterprises were in these two cities. I did face-to-face questionnaire interviews with general managers in the population from June 2003 to February 2004.¹ Because most of the CAS-owned enterprises were restructured in 1997–2002, the respondents’ recall of key individual information (e.g., social circles) and organizational information (e.g., profits) in general should be reliable.

Firms that had already registered as limited liability companies were not affected by the reform, so their general managers were excluded, together with three general managers who had started private firms before the reform. The whole population had 168 general managers, 134 of whom (80 percent) were successfully interviewed, which constituted the sample of this study. Out of the 51 laid-off respondents, 36 of them started a private business; by contrast, only 10 out of the 83 respondents who had not been laid off started their own business. Carroll and Mosakowski (1987) noted that most entrepreneurship studies examined only “starters,” that is, people who start a business, making it difficult to answer the question of why a person leaves an organization to start their own business. With both “starters” and “non-starters” in the sample, this study avoided this type of sample selection bias.

A major task of “Knowledge Innovation” was to restructure CAS-owned enterprises. Although CAS formally started “Knowledge Innovation” in 1998, the exact starting year varied in different institutes. It was a common Chinese practice that certain units experiment with a reform first, in keeping with Chinese leader Deng Xiaoping’s famous remarks—“cross the river by feeling the stones.” Eleven (8 percent) of the 134 enterprises experienced a similar reform before 1997; the rest (92 percent) of the enterprises in the sample were restructured in 1997–2002. Below, “the reform” refers to “Knowledge Innovation” or a similar reform.

I focused on immediate career choices following the reform, because a new context could elicit different mechanisms. For example, a person who left CAS and was employed by a company could start a private firm because of a different

“triggering event” that other people in this sample did not experience. I collected most organizational and individual information for the year prior to the reform (“benchmark year”). Since a private firm was started after the benchmark year, the causal path was relatively clear. Below, I describe the variables in statistical analysis; the related survey items are in the appendix.

Variables

Startup

Entrepreneurship refers to starting a private firm, namely, a limited liability company controlled by individuals or private firms. Because some of the limited liability companies in the sample had state-owned equity, I coded an interviewee as a private entrepreneur only if ALL of the following requirements were met in the limited liability company: (1) state-owned equity was less than one-third of the total (according to Chinese law, shareholders with one-third or more equity can veto major business decisions); (2) the state-owned equity portion was not the largest share; and (3) the respondent was one of the major shareholders. The strict coding ensured that the limit liability company was controlled by private parties, thereby minimizing the chance that CAS assigned a person to manage the company. While CAS superiors could decide whether a state-owned enterprise should be disbanded, they could not appoint a person to run a limited liability company in which CAS did not have a significant equity stake.

One-third of the 134 respondents ($n = 46$) started a private firm. Seven of the 46 private firms had minor CAS equity, 11 were founded by purchasing CAS-owned enterprises, and 28 were newly registered ones. Phillips (2002) and Romanelli (1985) noted that American entrepreneurs often launched firms whose business was similar to the business of their previous organizations, so this sample is not too peculiar to be compared to US samples. Studies of Chinese market transition usually put different types of private firms into one category (Wu and Xie 2003; Zhou, Tuma, and Moen 1997), which underlines their commonalities.

Starting a new firm via privatization seems less risky, but private entrepreneurs had to manage under a new property rights structure. According to Schumpeter (1934), entrepreneurs are individuals that carry out the new combination of means of production; privatization is a new combination of one of the most important means of production—property rights (Walder and Oi 1999). Schumpeter (1934) also highlights that having the freedom to make major decisions is an important feature of entrepreneurs, yet lacking such freedom had been a major problem for general managers of state-owned enterprises.

Furthermore, private firms faced greater political, regulatory, and financial restrictions than state-owned enterprises and multinational corporations (Huang 2008; Tsang 1994); private entrepreneurs had to learn how to survive in a business environment where state officials could turn to them for additional taxes/fees and even bribes (Ma and Parish 2006; Wank 1999); and private entrepreneurs must be fully responsible for their business decisions in an increasingly competitive market when CAS was not their last resort anymore. Therefore, launching a private firm is a critical turning point in these managers' career history.

Layoff

The respondents that were removed from the general manager position without any other job assignment were coded as “being laid off” (=1). I checked with the institute administrators to confirm the information given by the respondents. Laid-off managers could keep their CAS affiliation with partial or no pay, but they had no work to do. This style of layoff was the predominant form of unemployment in Chinese state organizations, which on the one hand had to downsize but on the other hand were obliged to ensure employment opportunities to their employees (Lee 2000).

There were three types of respondents—“stayers,” “movers,” and “starters.” “Starters” were people who started a private business, “stayers” were people who stayed in CAS after the reform,² and “movers” were those employed by another organization after the reform. Since there were only five “movers,” I treated them as “non-starters” in the binary logit regression analysis. The small number of “movers” revealed the impact of the “boss” identity on the choice of entrepreneurship, as one “starter” (#1) commented: “After that event [layoff], I had opportunities from an American company and a private firm. I did not take them because I do not want to take orders from others.” Another one (#61) said, “A few companies wanted me, but I do not want to be controlled.”

Social circles

Following surveys of small groups (Wuthnow 1994), I asked respondents to identify their social circles: “In the year prior to the reform, did you have any social circles composed of three or more people who were close to each other?” Because social circles were based on triadic strong ties that had a shared identity, it was easy for the respondents to recall them. To ensure recall accuracy, I asked the respondents to identify the year when a circle was formed and briefly describe the background information of its members. School ties were a major basis of affection-based social circles, and work ties (colleagues and business partners) were a major basis of business-based social circles.³ School ties were important for affection-based social circles because adolescent and young-adult friendships play crucial roles in the pursuit of identity and intimacy (Rawlins 1992), both of which may persist into adult life.

Before the questionnaire survey, I talked to 15 respondents in the sample, asking them about their social circles. Their comments, such as “we do not discuss business in this circle because business may contaminate friendship,” suggested that discussion topics could differentiate business-based and affection-based social circles. Bearman and Parigi (2004) found that trivial things such as the cloning of headless frogs were important topics in American core discussion networks. These trivial topics demonstrate that sociability helps maintain the liveliness, mutual understanding, and common consciousness of human groupings (Simmel 1964); this is probably why people considered mere chatter as important discussion.

For each social circle, I asked the respondent: “Did you usually discuss business in this circle, or was this circle related to or helpful for your business?” If the respondent gave an affirmative answer, the circle was coded a “business-based

social circle.” If the answer was negative, the circle was coded an affection-based one. For affection-based social circles, I asked respondents about the content of their interaction. They frequently said, “We only communicate *ganqing* [feelings]” and “Friendship is not sustainable if built from business relations.” It is difficult to precisely translate the Chinese word “*ganqing*,” which “exists only when sentiment, emotional attachment, and good feelings are felt by people involved in social interactions” (Yan 1996, 139). The respondents also noted regular contact with friends in affection-based social circles, suggesting that these circles could transcend life turbulences. A respondent (#88) commented on his affection-based social circle: “we stayed with each other through thick and thin during the ‘Cultural Revolution,’” one of the most turbulent times in Chinese modern history. Table 1 summarizes the respondents’ remarks on affection-based social circles.

The respondents’ remarks on business-based social circles were mainly about help for running the CAS-owned enterprise (e.g., “they provided technology as well as personnel help,” “they introduced clients to us,” or “we provided loan guarantees to each other”). Only one laid-off respondent received material help from an instrumental circle for launching his private business. A respondent (#2) recalled that after he left the general manager position, he lost contact with friends in his business-based social circle, likely because he was no longer useful to the circle. Another respondent (#29) was short of money for starting a private firm after being laid off. He approached a wealthy friend in his business-based social circle, but the friend refused to lend him money or invest in the startup. He eventually gave up the idea of starting his own business. These anecdotes suggest that business-based social circles might not be a reliable source of support during career crisis.

Table 1. Respondents’ Remarks on Affection-Based Social Circles

Resp. ID	Remarks
#8	“I have never thought about talking about business with them.”
#15	“I don’t want to bother my good friends [when I had career problems].” (*similar comment by #19)
#16	“Friends by nature shouldn’t be related to business” (*similar comment by #96)
#40	“We don’t talk about business because some friends are not doing well.”
#47	“We mainly communicate feelings.” (*similar comment by #48 and #55)
#58	“Doing business with friends is a taboo in our group.”
#82	“Friendship is not sustainable if built from business relations.”
#88	“We had stayed with each other through thick and thin during the ‘Cultural Revolution.’”
#89	“This group has absolutely no relationship with business.” (*similar comment by #1, #14, #72, #95, and #131)
#98	“Discussing business is out of the question.”
#121	“We don’t like business communication—nothing is sincere.”

Two “starters” received material help from friends in affection-based social circles (both of them were laid off). The small number ($n = 2$) suggests that explicit psychological support such as care and encouragement and implicit psychological support such as communication as usual might be affection-based social circles’ main contribution to private entrepreneurship ensuing after layoff. One “starter” (interviewee #7) recalled that he received enormous emotional support from affection-based social circles: “At that time, you felt many friends cared about you.” A respondent (#8) said that he had never thought about talking about business with friends in his affection-based social circle, but these friends did provide “private help” to one another, such as helping their friends’ kids go to a better school. His comment sheds light on the distinction between business exchange and reciprocal exchange (Molm 2010). Although friends in an affection-based social circle may provide material help to one another, the help is primarily for the receiver’s welfare rather than for the giver’s self-interest. The reciprocal exchange reinforces social solidarity rather than maximizes group members’ economic return.

I used two binary variables to index social circles—had at least one business-based social circle (= 1), and at least one affection-based social circle (= 1). I also tested the effects of the continuous measures of both types of social circles, that is, the number of each of the two types of social circles.

Structural holes

To further compare the moderating effects of sentimentality and instrumentality on the association between layoff and entrepreneurship, I analyzed the effect of “structural holes” (Burt 1992), a highly instrumental view of social networks. I used aggregate network constraint⁴ (Burt 1992) to measure “structural holes,” based on the matrix of two major types of business contacts, that is, management team members and major business partners of the CAS-owned enterprise in the year prior to the reform (see questions #8 and #13 in the appendix for the survey items). Similar to Burt (2004), I asked the respondent to report his/her perceived connection between each pair of people in the egocentric matrix. I used numerical scales to index the strength of each dyadic relationship: -2 = not harmonious; -1 = somewhat not harmonious; 0 = so-so; 1 = somewhat close; 2 = close (Bian 1997).

Spousal support

To compare affection-based social circles with other forms of emotional support, I included a variable indexing spousal support for starting a private firm:

-2 = did not support it; -1 = somewhat did not support it; 0 = neutral; 1 = somewhat supported it; 2 = supported it.

Control variables

Entrepreneurs often rely on personal funds to start their business (Evans and Jovanovic 1989), so the respondent’s monthly income was controlled after the adjustment for inflation. Because of the importance of the communist party in China (Dickson 2003), party membership (=1) was controlled. Whether a

respondent had been a CAS research group leader (typically a leading scientist) before being appointed a general manager was controlled because “star scientists” are crucial for technology entrepreneurship (Zucker and Darby 1996). I controlled for the number of employees that were hired from the labor market outside CAS (“non-CAS employees”) because they were similar to employees in private firms. The number of major partners in the private sector was controlled because they might be a pathway of influence for private entrepreneurship; the number of major partners in the state sector was controlled in that they might constrain the initiatives for starting a private business. The performance of the CAS-owned enterprise was measured by annual profits in the year prior to the reform, which may indicate the managerial capability of the respondent. The number of management team members of the CAS-owned enterprise was also controlled.

Results

I report binary logit models in which starting a private firm (= 1) is the dependent variable. Since 75 percent of the “starters” founded private businesses within 16 months and 95 percent of the “non-starters” had been “waiting” for more than 16 months by the time of my interview, most of the startup events might have occurred, suggesting that it is appropriate to use logit regression. Because a CAS institute usually had multiple enterprises, each of which had its own general manager, the logit models adopted robust standard errors, allowing for observations to be independent across institutes but not necessarily within them.

Descriptive Statistics

Table 2 reports summary statistics. The respondents’ average monthly income was 1.9 times the national average of people employed by state-owned enterprises (2.7 times for “starters”), and nearly two-thirds of the respondents were party members. One-third of the sample had at least one business-based social circle, and 45 percent of the sample had at least one affection-based social circle; all social circles had been formed before the reform. On average, there were four members in a business-based social circle and five in an affection-based one (excluding the respondent).⁵ The average length of association for affection-based circles was 28 years, and that for business-based ones was 16 years.

Multivariate Results

Tables 3a and 3b report the results of logit models. The VIF (variance inflation factor) scores for all the models fell far below the threshold of serious multicollinearity (typically 10). Model 1 includes all the control variables. The results of model 2 indicate that general managers who were laid off were 75 percent more likely to start a private firm than those who were not, with the other variables taking the sample mean. The significant effects of personal income and age were consistent with findings in the United States (Evans and Jovanovic 1989; Dunn and Holtz-Eakin 2000). Party membership did not have a significant effect, probably because the political advantages of party membership were restricted to CAS.

Table 2. Summary Statistics and Correlations

	Mean	SD	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
1. Startup (= 1)	.34	.48																
2. Age	50.00	9.54	-.09															
3. Party member	.63	.49	-.09	.12														
4. Research group leader (= 1)	.32	.47	.01	.31	-.03													
5. Personal monthly income (thous. of RMB)	3.05	3.36	.17	.12	.02	.09												
6. Number of non-CAS employees	15.39	30.06	.04	.03	.16	-.11	.17											
7. Number of major partners in private sector	.59	1.18	.20	-.06	.06	-.09	-.01	.02										
8. Number of major partners in state sector	2.51	2.53	-.05	.04	.07	.09	.24	.10	-.05									
9. Number of team members	1.45	1.50	-.05	.28	.15	.13	.12	.27	.13	.16								
10. Profits (mil. of RMB)	.62	2.02	.03	-.01	.09	-.01	.11	.52	-.08	.11	.20							
11. Laid off (= 1)	.38	.49	.60	.02	-.06	-.14	-.03	-.12	.14	-.05	-.17	-.14						
12. Had at least one business-based circle	.34	.48	.17	-.17	-.03	-.16	.10	.02	.05	.15	.06	-.09	.11					
13. Had at least one affection-based circle	.45	.50	.11	-.09	.14	-.07	-.11	.07	.05	-.02	-.14	.04	.07	.04				
14. # of business-based circles	.57	.90	.17	-.18	.06	-.17	.11	.04	.03	.09	.06	-.09	.12	.88	.07			
15. # of affection-based circles	.76	1.14	.10	-.20	.04	-.11	-.11	.14	.00	-.02	-.09	.36	-.05	-.06	.75	-.06		
16. "Structural holes"	.45	.40	.00	.08	.00	.01	-.12	.07	-.04	-.16	.14	.01	-.11	.03	.02	.09	.05	
17. Spousal support	.22	1.03	.14	-.20	-.08	.04	.11	.11	.06	.08	-.04	.09	.09	.18	.01	.20	.05	.01

Note: $N = 134$; * $p < .05$ if the absolute value of the correlation coefficient is greater than .17 (two-sided tests); RMB is the official Chinese currency. In 2001, the average monthly income for people employed by state-owned enterprises was 1,603 RMB in Beijing and 1,833 RMB in Shanghai, and the national average was 918 RMB (Chinese Statistics Yearbook). Both personal income and profits were in 2001 value.

Table 3a. Logit Models for Starting a Private Firm

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Age	-.02 (.02)	-.09* (.04)	-.08 [†] (.04)	-.10 [†] (.05)	-.07 (.05)	-.08 [†] (.05)
Party member	-.40 (.38)	-.21 (.43)	-.36 (.46)	-.39 (.47)	-.43 (.45)	-.66 (.54)
Research group leader	.15 (.45)	1.23 (.80)	1.31 [†] (.77)	1.48 [†] (.90)	1.34 [†] (.77)	1.39 [†] (.82)
Personal monthly income (logged)	.57 (.36)	1.14* (.46)	1.11* (.45)	1.03* (.44)	1.20* (.50)	1.11* (.49)
Number of non-CAS employees (logged)	.18 (.17)	.39* (.20)	.41 [†] (.21)	.50* (.24)	.37 [†] (.20)	.49* (.24)
Number of major partners in private sector	.37* (.18)	.21 (.15)	.20 [†] (.16)	.24 (.18)	.25 (.18)	.29 (.20)
Number of major partners in state sector	-.05 (.08)	-.15 (.11)	-.17 (.11)	-.21 [†] (.11)	-.15 (.10)	-.15 (.10)
Number of team members	-.13 (.16)	.09 (.17)	.14 (.19)	.06 (.20)	.15 (.19)	.15 (.19)
Profits (logged)	-.05 (.05)	-.01 (.06)	.00 (.06)	.00 (.06)	-.02 (.06)	-.04 (.06)
Laid off		4.13*** (.68)	4.21*** (.69)	3.12*** (.85)	4.39*** (.73)	3.26*** (.84)
Had at least one business-based circle			.36 (.58)	.35 (.86)		
Had at least one affection-based circle			.82 (.66)	-.60 (1.04)		
# of business-based circles					.32 (.26)	.01 (.34)
# of affection-based circles					.52* (.24)	.24 (.27)
Layoff × Had at least one business-based circle				.02 (1.07)		
Layoff × Had at least one affection-based circle				2.52* (1.18)		
Layoff × # of business-based circles						.66 (.56)
Layoff × # of affection-based circles						1.21* (.53)
N	134	134	134	134	134	134

Table 3b. Logit Models for Starting ■ Private Firm

	Model 7	Model 8	Model 9	Model 10
Age	-.09* (.04)	-.12* (.06)	-.08† (.04)	-.10† (.05)
Party member	-.44 (.46)	-.47 (.45)	-.41 (.45)	-.40 (.46)
Research group leader	1.25 (.79)	1.48 (1.02)	1.19 (.85)	1.40 (.95)
Personal monthly income (logged)	1.32** (.50)	1.23* (.48)	1.17* (.46)	1.11* (.46)
Number of non-CAS employees (logged)	.43* (.19)	.55* (.24)	.41* (.20)	.51* (.22)
Number of major partners in private sector	.25 (.16)	.31* (.20)	.20† (.16)	.23* (.17)
Number of major partners in state sector	-.13 (.10)	-.17 (.12)	-.16 (.11)	-.22† (.12)
Number of team members	.09 (.22)	-.02 (.26)	.15 (.19)	.05 (.19)
Profits (logged)	-.02 (.06)	-.01 (.06)	.00 (.06)	.00 (.06)
Laid off	4.41*** (.70)	3.39** (1.14)	4.24*** (.68)	3.20*** (.76)
Had at least one affection- based circle	.83 (.66)	-.73 (1.09)	.84 (.66)	-.69 (.97)
“Structural holes”	.98 (.71)	1.39 (.90)		
Spousal support			.11 (.31)	.32 (.57)
Layoff × Had at least one affection-based circle		2.80* (1.33)		2.61* (1.15)
Layoff × “Structural holes”		-.31 (1.45)		
Layoff × Spousal support				-.25 (.59)
N	134	134	134	134

Note: Since clustering is used to control for interdependent observations (i.e., firms belonged to the same institute), conventional likelihood-ratio test is inappropriate. STATA (the software used in the reported analysis) recommends using a Wald test, which suggests that including “layoff × at least one affection-based circle” in model 4 significantly improved the fit of the model ($p = .03$), but the inclusion of “layoff × at least one business-based circle” did not significantly improve the model fit.
 *** $p < .001$ ** $p < .01$ * $p < .05$ † $p < .10$. Two-sided tests. Robust standard errors are in parentheses.

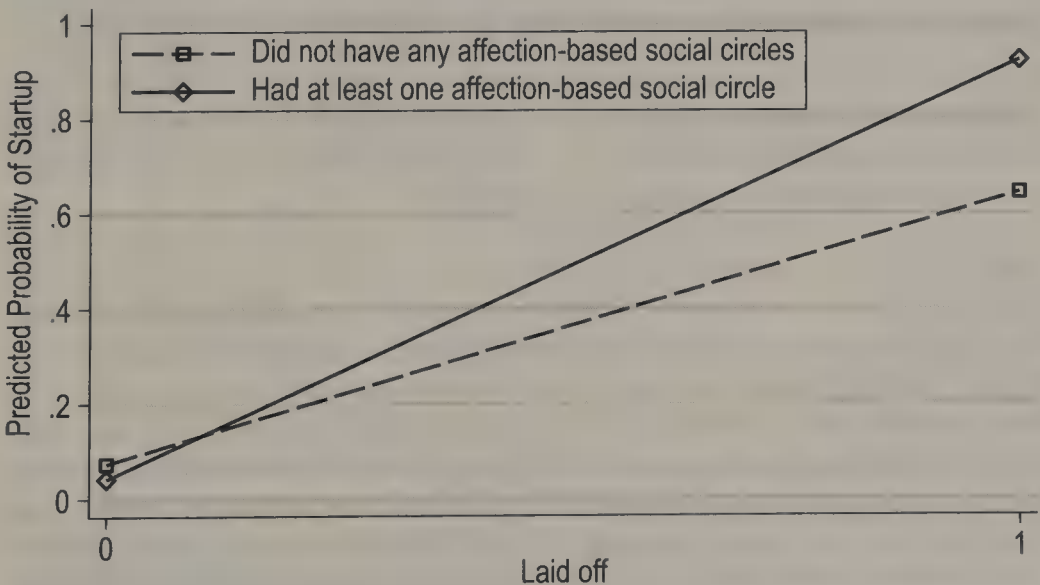
In model 3, the main effects of whether the respondent had a business-based social circle and whether the respondent had an affection-based social circle were insignificant. Model 4 indicates that having at least one affection-based social circle significantly moderated the effect of layoff on private entrepreneurship, whereas having at least one business-based circle did not have a significant moderating effect.⁶ Whereas business-based social circles neutralized the positive effect of layoff on entrepreneurship, affection-based social circles strengthened it. Based on Long and Freese's (2006) codes for graphing interactions, figure 2 shows that the positive relationship between layoff and startup was stronger for managers with at least one affection-based social circle than for those who did not have any affection-based social circles.⁷

I used the binary variables of affection-based and business-based social circles for the main analysis because of their skewed distributions (the 75th percentile of the distribution of both variables was one circle). In model 6, I examined the interaction between layoff and the continuous measure of each of these two kinds of social circles; the results were similar to those of model 4.

Model 7 adds the main effect of "structural holes," and model 8 includes its interaction with layoff. "Structural holes" did not significantly moderate the impact of layoff on private entrepreneurship, and the moderating effect of affection-based social circles remained significant (model 8). Although the ego connecting otherwise unconnected alters has information and control benefits (Burt 1992), it is difficult for "structural holes" to be a source of identity support (Podolny and Baron 1997). When actors surrounding the ego are separate or loosely linked, social belonging may be so weak that the ego can end up being a lonely person struggling with distress after layoff.

Model 9 adds the main effect of spousal support, and model 10 includes its interaction with layoff. Whereas spousal support did not significantly moderate the impact of layoff on starting a private firm, the moderating effect of

Figure 2. Interaction of layoff and affection-based social circles



affection-based social circles remained significant (model 10). Affection-based social circles may be more effective than family ties in facilitating entrepreneurship following layoff, probably because the distressful event impeded emotional support from family ties (Price, Friedland, and Vinokur 1998). For example, his identity as a breadwinner may create severe distress for a father who loses his job, despite the sincere emotional support that his wife provides.

To treat the potential unequal residual variation between different groups (Allison 1999), I ran logit regression only among those who were laid off, and found that having at least one affection-based social circle had a significant main effect on private entrepreneurship (model 11), and the continuous measure of affection-based social circles had a significant effect as well (model 12). The effect of business-based social circles stayed insignificant, and neither “structural holes” (model 13) nor spousal support (model 14) had a significant effect. Together, the statistical evidence indicates that general managers with affection-based social circles were more likely to start their own business after layoff.

Robustness Checks

In a separate analysis, I added a binary variable indicating that the reform occurred before 1997 to the models in tables 3 and 4; the results were similar to the reported ones. I also used a binary variable of “reform before 1998” and obtained similar results. I replaced “starting a private firm” ($n = 46$) with “starting a newly registered private firm” ($n = 28$), and the results were consistent. I tested the interaction of layoff and the binary variable of whether the respondent had any social circles that include both affection-based and business-based ones; the interactive effect was insignificant, suggesting that disaggregating social circles into affection-based and business-based ones enhances inference.

Since social circles might affect the chance of being laid off as well as the likelihood of starting a private business, I tried a two-equation probit model (Greene 2000) that allows correlated disturbances. The result of the equation in which “starting a private firm” was the dependent variable was consistent with that of model 4, and the other equation suggested that none of the two types of social circles had a significant impact on layoff. The Wald test of the independence between the two equations (H_0 : the two equations were independent of each other) had a chi-square score of 3.49 ($p = .06$), suggesting that the error terms of the two equations were only marginally correlated.

Demotion (i.e., CAS assigned an inferior position to a respondent) is another form of career blockage. I added the interaction between demotion ($n = 22$) and each of the two types of social circles to model 4. While none of the two interactive effects were significant, the interaction between layoff and affection-based social circles remained significant and the interaction between layoff and business-based social circles remained insignificant. I also checked the main effect of gender (91 percent of the sample were males), education, scientific division of the CAS institutes (basic sciences, biological sciences, resources and environmental sciences, or high-tech sciences), geographical location (Beijing versus Shanghai), and whether the respondent had seriously considered starting a private business before the reform. None of them had a significant effect, and dropping them did not significantly influence the model fit.

Table 4. Logit Models for Starting a Private Firm among Laid-Off Respondents

	Model 11	Model 12	Model 13	Model 14
Age	-.17 (.15)	-.17 (.19)	-.16 (.13)	-.17 (.14)
Party member	-1.13 [†] (.66)	-1.45 (.96)	-1.17 [†] (.64)	-1.43 [†] (.79)
Research group leader	.01 (1.38)	.19 (1.48)	-.52 (1.32)	-.49 (1.32)
Personal monthly income (logged)	1.63 [†] (.88)	1.64 (1.16)	2.03** (.74)	1.74* (.89)
Number of non-CAS employees (logged)	.57 (.53)	.65 (.60)	.66 (.49)	.60 (.51)
Number of major partners in private sector	.65** (.24)	.81* (.34)	.71*** (.22)	.69** (.24)
Number of major partners in state sector	-.19 (.23)	-.11 (.22)	-.17 (.18)	-.23 (.21)
Number of team members	.26 (.31)	.27 (.32)	.18 (.31)	.24 (.28)
Profits (logged)	-.04 (.08)	-.12 (.10)	-.09 (.10)	-.04 (.08)
Had at least one business- based circle	.14 (1.06)			
Had at least one affection-based circle	2.47** (.96)		2.58** (.92)	2.64** (.88)
# of business-based circles		.71 (.66)		
# of affection-based circles		1.93* (.86)		
"Structural holes"			1.55 (1.33)	
Spousal support				.40 (.38)
N	51	51	51	51

*** $p < .001$ ** $p < .01$ * $p < .05$ [†] $p < .10$. Two-sided tests. Robust standard errors are in parentheses.

To further explore the impact of political capital on private entrepreneurship, I tested the interactive effect of party membership and layoff on starting a private firm and did not find a significant effect, with the effects of the other variables being similar to the reported ones. I also addressed authority relations, that is, the average strength of the relationship between the respondent and each of the CAS superiors before the reform began (for each dyad: -2 = not harmonious; -1 = somewhat non-harmonious; 0 = so-so; 1 = somewhat close; 2 = close). Authority relations did not have a significant main effect; its interactive effect with layoff

was also insignificant. The effects of the other variables were consistent with the reported ones. Four respondents (three of them were laid off) had a CAS superior in their business-based circles, and four respondents (two of them were laid off) had a CAS superior in their affection-based social circles. Because very few social circles had CAS superiors, the chance that CAS administrators helped the careers of their friends in a social circle might have been low. It would be interesting to further investigate the relationship between social circles and authority relations in future studies (e.g., analyze a sample where there are more superiors in social circles).

Discussion

Theoretical Implications

This study highlights the imperative for investigating the relationship between economic action and social relations that primarily build on sentimentality, and sheds light on entrepreneurship research's contribution to social network analysis. Podolny (2001) split network analysis of embeddedness into two streams—networks as pipes through which information and opportunities flow, and networks as prisms inducing differentiation among actors. Affection-based social circles are like yo-yo strings that reintegrate economic actors in disarray back into society. When job losers felt abandoned by their organization, affection-based social circles were a harbor where they found undiminished self-worth.

Zelizer (2002) identified two perspectives regarding the relationship between sentimentality and instrumentality—the “Hostile-Worlds” view sees them as segregated domains that require well-maintained boundaries, while the “Nothing-But” view argues that the ostensibly separate world of intimate social relations is nothing but a special case of economic, cultural, or political principles. This study rejects a “Nothing-But” view that reduces sentimentality to economic rationality, yet it also moves beyond the “Hostile-Worlds” view because the demarcation between social and economic spheres helps integrate the two spheres at a deeper level. The demarcation shields self-identity from career turbulence, enabling laid-off general managers to confidently pursue a radical career path of private entrepreneurship.

This study incorporates institutional contexts into embeddedness research (Granovetter 2002). In the context of Chinese market transition, drastic organizational structuring shattered the bureaucratic path that general managers of state-owned enterprises were used to. When laid-off managers lost organizational belonging, affection-based social circles filled the void and helped them transition into private entrepreneurship. Affection-based social circles not only connect the economic sphere with the social sphere through social belonging, but also let the social sphere remain autonomous rather than being an epiphenomenon of the market (Polanyi 1957).

Limitations and Avenues for Future Research

The respondents demonstrated good memories of the information that I requested, yet recall inaccuracy was still possible in the retrospective survey. While this study focuses on the *structural* properties of social support, that is, affection-based versus

business-based social circles, it is important to investigate the *functional* properties of social support, such as the perceived amount and adequacy of emotional support, magnitude of self-esteem and mastery, self-affirmation, and so forth (Thoits 1982; Wiesenfeld et al. 2001). The further investigation can not only provide a more direct test for the association between social circles and identity support, but also illuminate how structural and functional aspects of social support are related to each other (Thoits 1995). It is also possible that certain personality traits, for example charming resilience, could help a person form an affection-based social circle as well as overcoming the psychological distress resulting from layoff. It is important to address this potential selection effect in future studies.

Besides friendship ties, it is important to investigate two other forms of the primary group, neighborhood and family/kinship ties (Cooley 1962). Like other large state organizations, CAS had its own residential clusters where most respondents in this study lived, such that it was difficult to separate neighborhood ties from work ties (Shenkar 1996). It is important to investigate neighborhood ties in a different context. In addition to spousal support, it is worth investigating the effect of other family/kin ties (Renzulli, Aldrich, and Moody 2000) to examine whether ascriptive family/kin ties may constrain entrepreneurial initiatives through “censorship” (Rubin 1985) while affection-based circles supply identity support without confining individual freedom.

On average, each respondent had been in the general manager position for 5.4 years before the reform. Because some of the respondents may not have had enough time to develop business-based social circles, it is important to analyze a sample with a longer time span of business exposure. It is also worthwhile to collect data for testing the effect of affection-based social circles on the performance of startups to examine whether these circles continue to supply the social belonging that entrepreneurs need (Shepherd and Haynie 2009).

With respect to the generalizability of this study, the layoffs documented by this study stemmed from drastic institutional transformation in which capable managers could lose their job. This kind of layoffs is comparable to massive layoffs in the latest US economic crisis (Gibson 2012), which to a large extent arose from institutional problems (Davis 2011). Farr (1988) reported an American primary group (“good old boys sociability groups”—GOBS groups) in which men retreat from the world of work and family. Since members in GOBS groups typically engage in activities for fun and pleasure, these groups were likely affection-based social circles. Similarly, Wuthnow (1994) highlighted the small-group movement in America, which springs from the breakdown of the traditional support structures and from human beings’ enduring desire for community. These small groups (Farr 1988; Wuthnow 1994) provide promising opportunities for investigating the relationship between affection-based social circles and entrepreneurship in the United States, although future researchers need to keep in mind that similar social structures may contain different cultural contents.

Conclusion

Although this study reveals the distinction between affection-based and business-based social circles, we should not assume that they can never be converted into

each other. On a cautious note, as China goes deeper into market transition, the increasing commodification of social relationships may cause the decay of the primary group. As people instrumentalize affection-based social circles, they may unwittingly dislocate themselves from a vital source of social belonging. History reminds us that an all-encompassing economic system (e.g., the 19th-century capitalist economy; see Polanyi [1957]) or political system (e.g., 20th-century socialist experiments) may collapse because of the dislocation of social autonomy. It is important for us to remember these lessons during contemporary social change.

Notes

1. For general managers that no longer worked for CAS, I managed to get their mobile phone numbers to set up the interview. To facilitate the retrospective interview, I socialized with the respondents in such ways as going out for dinner and organizing seminars for them to discuss managerial issues. The interviews were conducted in the interviewee's office when nobody else was present, in a café, or in a restaurant.
2. Ten respondents retired after the reform. They were "stayers" because their retirement benefits were administered by CAS and they continued to participate in CAS activities.
3. An alternative approach to generate a social circle is to let the respondent identify a set of people in the "network universe" and then use an algorithm to find triads that meet the requirement of a clique, namely a maximal subgraph of three or more nodes (Wasserman and Faust 1994). This approach cannot capture the content of ties such that a shared identity may be missing. If A and B are intimate neighbors, B and C are close classmates, and A and C are close church friends, the three satisfy the requirement for a clique but they may not know each other.
4. A higher constraint score indicates greater network redundancy. Contact j 's constraint on ego is defined as $C_j = (p_{ij} + \sum_q p_{iq} p_{qj})^2$, $q \neq i, j$, where p_{qj} is the proportional strength of q 's relationship with j , p_{iq} is the proportional strength of i 's relationship with q , and p_{ij} is the proportional strength of i 's relationship with j . The sum of C_j across contact j measures the aggregate constraint on the ego's entrepreneurial opportunities within the network (Burt 1992).
5. There were four pairs of overlapping social circles, each of which shared only one person, revealing cognitively demarcated social spaces. Eight out of the 181 circles in this sample had mixed relations (e.g., one person had business relations with the respondent, whereas the others did not). The majority rule was used to code these circles. For example, one member in a circle of 11 people had a business relationship with the respondent; the circle was coded as an affection-based one.
6. Regression diagnostics of model 4 indicate that only one of the 134 observations is relatively far away from the other observations. The results were similar if the outlier is removed from the sample.
7. Power analysis (Agresti 2002) for model 4 indicates that the model needs a sample size of 134 to achieve a statistical power of .90. The sample size of this study was exactly 134.

Appendix. Survey Items Used for the Variables

*Below, “the reform” refers to “Knowledge Innovation” or a similar reform.

**Explanations of the coding procedure and related information are in square brackets. The order of the questions below is the same as the order in tables 2–4 but different from the order in the actual survey (e.g., startup information was requested after the respondent provided basic information about themselves and the CAS-owned enterprise that he/she managed).

1. “What year did the reform start in your institute?”
2. Startup: “Did you start a private firm after the reform began? A private firm refers to a limited liability company controlled by individuals or private firms. Please provide ownership information of the company.”
[If a limited liability company had state-owned equity, it had to meet ALL of the following requirements to qualify for a private firm: (1) state-owned equity was less than 1/3 of the total; (2) the state-owned equity portion was not the largest share; and (3) the respondent was one of the major shareholders.]
3. Age: “What year were you born?”
[Age = the year when the reform began – birth year]
4. Party membership: “Were you a party member?” “What year did you join the party?”
[All party members joined the party before the reform.]
5. Research group leader: “What was your job before being appointed general manager of the CAS-owned enterprise?”
[If the respondent mentioned research group leader, the variable was coded 1; otherwise, it was coded 0.]
6. Personal monthly income: “What was your monthly income in the year prior to the reform?”
[The consumer price index was used to adjust inflation.]
7. Number of non-CAS employees: “In the year prior to the reform, how many employees in the CAS-owned enterprise that you managed were hired from the labor market outside CAS?”
8. Number of major partners in the private/state sector: “In the year prior to the reform, who were major partners of the CAS-owned enterprise that you managed? You only need to provide their last name. Please describe their job. Major partners refer to the most important people in major partnership organizations (e.g., suppliers, customers, banks, government agencies) of the CAS-owned enterprise. If the enterprise had many suppliers or customers, to qualify for a major partnership organization, the transaction between them and the CAS-owned enterprise had to be at least 10% of the CAS-owned firm’s annual revenue. If you knew several people in a major partnership organization, please identify the most important one.”
[If a major partner was in a private firm, the partner was coded as being in the private sector; if a major partner was in a state organization, the partner was coded as being in the state sector.]

9. Number of team members: “In the year prior to the reform, who were your management team members of the CAS-owned enterprise? You only need to provide their last name.”

[I asked them to identify the specific persons in their major partner organization and management team in order to construct the network matrix for computing “structural holes.”]

10. Profits: “In the year prior to the reform, how much profit did the CAS-owned enterprise that you managed make?”

[The producer price index was used to adjust inflation.]

11. Laid off: “When the reform began, what was the job arrangement that the CAS institute gave you?”

[If the respondent was removed from the general manager position without any other job assignment, he/she was coded as “being laid off” (= 1).]

12. Social circles: “In the year prior to the reform, did you have any social circles composed of three or more people who were close to each other?” [If the respondent had a social circle] “did you usually discuss business in this circle, or was this circle related to or helpful for your business?”

[If the respondent gave an affirmative answer to the second question, the circle was coded a “business-based social circle.” If the answer was negative, the circle was coded an “affection-based social circle.” To ensure recall accuracy, I asked respondents the year when a circle was formed; they were also asked to briefly describe each member in a social circle. To verify whether a circle was primarily based on sentimentality or instrumentality, the respondents were invited to provide information about the typical content of discussions in each social circle.]

13. “Structural holes”: “Could you tell me the relationship between each of the person in the following matrix which is composed of your major partners and management team members?”

[They were asked to report the strength of each dyadic relationship: -2 = not harmonious; -1 = somewhat not harmonious; 0 = so-so; 1 = somewhat close; 2 = close.]

Below is an example of the network matrix:

	Self	Partner 1	Partner 2	Partner 3	Teammate 1	Teammate 2
Self						
Partner 1						
Partner 2						
Partner 3						
Teammate 1						
Teammate 2						

14. Spousal support: “What was your spouse’s attitude about starting a private business?”

[-2 = did not support it; -1 = somewhat did not support it; 0 = neutral; 1 = somewhat supported it; 2 = supported it]

About the Author

Dali Ma is an associate professor of management at Drexel University. He received his PhD in sociology from the University of Chicago and is interested in economic and organizational sociology. His research has been published in *Social Forces* and *Academy of Management Journal*.

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Expectations on Track? High School Tracking and Adolescent Educational Expectations

Kristian Bernt Karlson, *University of Copenhagen*

This paper examines the role of adaptation in expectation formation processes by analyzing how educational tracking in high schools affects adolescents' educational expectations. I argue that adolescents view track placement as a signal about their academic abilities and respond to it in terms of modifying their educational expectations. Applying a difference-in-differences approach to the National Educational Longitudinal Study of 1988, I find that being placed in an advanced or honors class in high school positively affects adolescents' expectations, particularly if placement is consistent across subjects and if placement contradicts tracking experiences in middle school. My findings support the hypothesis that adolescents adapt their educational expectations to ability signals sent by schools.

Introduction

Many important factors contribute to inequalities in educational attainment, among them educational expectations. Sociologists of education have long regarded educational expectations as a key component in models of status attainment (Sewell, Haller, and Portes 1969; Otto and Haller 1979), and have examined the psychological consequences of unmet expectations (Hanson 1994; Reynolds and Baird 2010). Even with overall increases in adolescents' educational expectations over the past three decades (Goyette 2008; Reynolds et al. 2006), studies show that expectations continue to be strongly linked to educational attainment (Morgan 2004, 2005).

Despite the well-documented effects of educational expectations on attainment, researchers debate how adolescents form expectations about their future (Andrew and Hauser 2011; Bozick et al. 2010; Manski 1993; Morgan 1998). Scholars disagree on the extent to which adolescents modify their educational expectations in the light of new information about their academic abilities.

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However, most studies on expectation formation focus on the information about academic abilities that adolescents receive from grades; therefore, they tend to ignore other ability signals that schools send them. One such signal is educational tracking. Previous research suggests that educational tracking, because it clearly differentiates chances for future success, is associated with cultural beliefs about academic competence and the legitimate entitlement of social status (e.g., Oakes 1985; Sørensen 1984). Consequently, adolescents may have good reasons for modifying their educational expectations in response to their track placement.

This paper analyzes the role of high school tracking in the formation of adolescents' educational expectations and makes three contributions to the literature. First, in contrast to previous studies, I use educational tracking to test the hypothesis that adolescents adapt their educational expectations to new information about their academic abilities. Second, I extend previous research by arguing that adolescents differ according to the extent by which high school track placement reveals *new* and *consistent* information to them. As most adolescents are tracked prior to high school, I expect the informational effects of high school tracking to depend on previous tracking experience. Moreover, a considerable fraction of adolescents may experience contradictory or fuzzy track signals, because they are in a high-track class in one subject and in a low-track class in another. I therefore expect adolescents to respond more strongly to consistent, as opposed to discrepant, track placement across different subjects. Third, to handle the issue of selectivity bias in the estimation of tracking effects, I apply a difference-in-differences approach to the National Educational Longitudinal Study of 1988. As this approach controls for stable, unmeasured characteristics of adolescents, it allows me to isolate the signaling effect of track placement in high school.

My empirical analysis largely corroborates the theoretical predictions. Adolescents appear to adapt their educational expectations to the ability signals sent by the track placement in high school. The extent to which adolescents respond to their high school track placement depends, first, on whether their placement is consistent across subjects and, second, on their prior tracking experience. These findings support the position that emphasizes processes of adaptation in the formation of adolescents' educational expectations, and point to the need for considering the role of prior academic experiences in expectation formation processes.

Background

Performance Feedback and the Formation of Educational Expectations

Research in educational stratification has long documented the positive consequences of adolescents holding high hopes for their future educational attainments (Sewell, Haller, and Portes 1969; Morgan 2004). Expecting to complete college not only mediates a substantial portion of the socioeconomic gradient in educational attainment but also propels schooling choices independent of family background and previously demonstrated abilities. Scholars nevertheless disagree on the origin and nature of adolescents' expectations for the future. In the status attainment tradition, which has dominated much of the sociological writing on

this subject, adolescents' expectations are the outcome of the early socializing influences of significant others (Haller 1982; Haller and Portes 1973; Otto and Haller 1979). Children internalize the achievement expectations that significant others hold for them as a static mental construct that, once crystalized, comes to motivate schooling behaviors and decisions (Andrew and Hauser 2011).

A contrasting perspective, which has regained momentum in recent years, challenges the view that adolescents' expectations are the sole outcome of early socialization. This perspective argues that expectations also reflect the organization of society's opportunity structure and, not least, the individual's perceived chances for success in this structure (Kerckhoff 1976; Gambetta 1987; MacLeod 1987; Ogbu 1978; Willis 1981). Recent literature—attempts to clarify the micro-foundations of educational decision-making—argues along similar lines that adolescents' educational decisions are guided by their appraisals of the outcomes of and their chances for success in future schooling (Breen and Goldthorpe 1997; Morgan 2005). This burgeoning literature assumes that adolescents make their appraisals in response to the continuing provision of information from various sources, such as parents, teachers, peers, and the mass media (Bozick et al. 2010; Breen 1999; Grodsky and Jones 2007; Morgan 2005; Rosenbaum 2001; Schneider and Stevenson 1999).¹ Yet, among the most pertinent sources of information available to adolescents is the ongoing feedback on their academic abilities, provided by institutionalized performance indicators in schools (Bozick et al. 2010). As these indicators likely assist the adolescent in determining his or her chances for success in future education (Covington 1992), adolescents may have good reasons for regulating their educational expectations in light of the new information these indicators convey.

Nonetheless, existing research provides mixed support for the hypothesis that adolescents adapt their educational expectations to new information about their academic abilities. In a recent study, Andrew and Hauser (2011) find that adolescents appear to largely disregard new information about their academic standing when forming educational expectations. Andrew and Hauser therefore conclude that expectations stabilize before adolescence and are rather persistent over time. In support of this conclusion, using retrospective adolescent reports, Grodsky and Riegle-Crumb (2010) report that many students from a young age take college enrollment for granted. Although these findings appear to confirm the lasting importance of the early formation of expectations, recent studies in the economics of education provide evidence to the contrary. They find that learning about academic abilities in college plays an important role in the formation of expectations and ultimately in college major and dropout decisions (Stinebrickner and Stinebrickner 2009, 2011; Zafar 2011). Similarly, studies in both sociology and economics report that adolescents' revisions of expectations over their educational career are a result of learning about their academic abilities (Bozick et al. 2010; Jacob and Wilder 2011).²

In sum, studies on expectation formation appear to disagree about the extent to which new information about academic abilities leads adolescents to revise the expectations they hold for their future. However, most of these studies focus narrowly on how the grade point average (GPA) conveys information about abilities. While GPAs are an important source of information, schools have other channels

for sorting adolescents according to academic abilities. One such channel is educational tracking, which entails the organizational differentiation of learning opportunities by which adolescents are divided into tracks or groups for instructional purposes (Sørensen 1970). To examine the role of adaptation in adolescents' formation of educational expectations, in this paper I test whether high school track placement affects educational expectations.

The Complexities of Track Signals

Research on educational tracking in secondary schools offers at least three mechanisms through which we can expect track placement to affect adolescents' educational expectations. First, as tracking differentiates opportunities for learning, high-track adolescents are likely to learn more than low-track adolescents (Gamoran 1986). Insofar as learning affects expectations, the effects of track placement on expectations may reflect this learning mechanism. Second, tracking may affect expectations through its stratification of peer group memberships (Hallinan and Sørensen 1985). Peers are widely regarded as influencing adolescents' orientations and self-concepts via prevailing aspirational norms and group-specific evaluation standards (Hallinan and Williams 1990; Kelley 1952). Whenever track-dependent peer influences lead adolescents to adjust their educational expectations, the track effects will pick up this adaptation of expectations to the surrounding social environment.

Third, ethnographic studies find that educational tracking involves social labeling processes that are rooted in cultural assumptions about the role of individual competence (LeTendre, Hofer, and Shimizu 2003; Oakes et al. 1997; Schwartz 1981). As Oakes (1985, 3) puts it, "A student in a high-achieving group is seen as a high-achieving *person*, bright, smart, quick, and in the eyes of many, good [*italics in original*]." Because track placement makes publically visible the opportunities of achieving success in the educational system, it conveys a signal to the adolescents about their academic potential, independent of their actual abilities (Gamoran 1986; Sørensen 1984). Insofar as adolescents respond to this signal, I expect track placement to affect educational expectations via the social labeling processes caused by curricular differentiation.

Previous tracking studies support the hypothesis that track placement affects expectations, even after controlling for expectations at the beginning of high school (Alexander and Cook 1982; Wiatrowski et al. 1982; Vanfossen, James, and Spade 1987). Studies also find that adolescents, when forming expectations, rely heavily on the cues conveyed by the tracking structure, leaving little room for peer influences (Shavit and Williams 1985; Yuchtman and Samuel 1975). This latter evidence therefore suggests that the direct signaling effect of track placement, as implied by the social labeling explanation, dominates the indirect effects that operate via learning and peer influences. Nevertheless, despite this evidence, previous research has not examined two issues that have substantive implications for the study of the signaling value of track placement in expectation formation processes.

First, tracking practices in secondary schools have changed markedly over the past four decades, rendering the signaling value of track placement more

ambiguous than earlier. Today, high school students enroll in courses, not in over-arching programs. Yet, whereas they have some degree of freedom in choosing their subjects, they have less control over their placement in the stratified curriculum within subjects (Lucas 1999).³ As a result, a considerable fraction of adolescents experience discrepant course placements across different subjects (Kelly and Carbonaro 2012). To the degree that adolescents glean information about their academic abilities from their course placements, this group of adolescents may experience their mixed placements as providing a fuzzier signal about their academic abilities and consequent future opportunities (compared to those who are consistently placed in high- or low-track courses).

Second, previous research has put little effort into theoretically identifying the adolescents for whom high school track placement can be considered as revealing *new* information to them about their academic abilities. Most adolescents are exposed to tracking before entering high school and thus have an idea about which track they belong in. Because track placement in high school can either corroborate or conflict with this idea, adolescents are likely to respond in different ways to the ability signals conveyed by track placement in high school. For adolescents staying in the same track in the transition from middle to high school, their high school track placement will tend to confirm their initial beliefs about their academic abilities. Thus, from the perspective of the adolescent, track placement does not reveal new information about his or her academic abilities. In contrast, for those changing tracks during the transition, high school track placement will tend to disconfirm their initial beliefs, consequently providing new information to them about their academic abilities.

Appreciating the complex ability signals that track placements convey has consequences for any proper evaluation of the degree to which adolescents adapt their educational expectations to the information implied by their high school track placement. On the one hand, as the informational value of track signals depends on the consistency of placements across disparate subjects, adolescents should exhibit stronger adaptation to consistent, not discrepant, course placements. On the other hand, as only adolescents changing tracks from middle to high school can meaningfully be said to receive new information about their abilities, evaluating track placement effects requires differentiating between movers and stayers in the stratified curriculum during the transition to high school. Thus, in the empirical analyses in this paper, I test whether adolescents differ in their response to the signals they receive according to both the consistency of their course placements and their previous tracking experiences in middle school.

Data and Methods

Sample

For the empirical analysis, I use the eighth- and tenth-grade cohorts in the Public Use Version of the National Educational Longitudinal Study of 1988 (NELS) (Curtin et al. 2002). In contrast to other surveys, NELS provides longitudinal information on adolescents' educational expectations and tracking experiences both before (eighth grade) and after (tenth grade) adolescents are tracked in high

school. This unique design allows me to study, first, whether track differences in educational expectations in high school were in place before adolescents enter high school and, second, how the effect of high school track placement on expectations depends on tracking experiences in middle school.

NELS is a national probability sample following 25,000 eighth graders in 1,000 schools from 1988 through 2000. I use those 17,184 adolescents who are observed in both of the first two waves (1988 and 1990). For each adolescent, NELS provides teacher reports on high school course placements in two out of four subjects (Ingels et al. 1992). This feature of the data means that track information for each course is available only for different random subsamples of adolescents, subsamples that only partially overlap. In my analyses, I therefore examine curricular placements in mathematics and English, because these two subjects provide the largest possible overlap of random subsamples and because previous research has analyzed adolescents' joint placements in the two subjects (e.g., Lucas 1999). Given substantial nonresponse on control variables, I combine multiple imputation with inverse probability weighting to increase efficiency and to restore generalizability of the analyzed subsamples.⁴ Because NELS uses a complex sampling design, I also use the panel weight available in NELS (Curtin et al. 2002), and I correct standard errors for the stratified design and clustering of observations both within eighth-grade school units and within individuals over time.

The final samples comprise 6,013 and 7,217 adolescents in mathematics and English, respectively, with 3,169 adolescents overlapping in the combined placement sample. In each of these samples, each adolescent is observed twice, in the eighth and tenth grade.⁵ Although these samples comprise only partially overlapping adolescents, table 1 shows that the composition of the samples is very similar in terms of the distributions of the control variables used in the analyses. Additional calculations, which I do not report here, show that this pattern also holds for background characteristics such as parental socioeconomic status, race, and gender. These calculations also show that the social composition of the samples resembles that of the full NELS sample covering all youth observed in both of the first two waves.

Educational Expectations

In each of the two first waves of NELS, adolescents are asked about their expected level of educational attainment: "As things stand now, how far in school do you think you will get?" Following previous studies on educational expectations (Morgan 1998; Andrew and Hauser 2011), I code these responses into years of education. On average, adolescents expect to attain about 16 years of education, which is equivalent to completing a four-year college degree, and this average does not change from eighth to tenth grade (table 1). However, the standard deviation in expectations increases from about 2.10 years to about 2.25 years from eighth to tenth grade, suggesting a widening dispersion in expectations. Further calculations reveal that about 54 percent of the variation in expectations over time lies between individuals, indicating that a considerable fraction of adolescents revise their expectations during the transition to high school.

Table 1. Means and Standard Deviations for the Variables in the Analysis

	Mathematics		English		Combined placement	
	Eighth grade (1988)	Tenth grade (1990)	Eighth grade (1988)	Tenth grade (1990)	Eighth grade (1988)	Tenth grade (1990)
Expectations in years	15.974 (2.103)	15.971 (2.260)	15.993 (2.097)	16.002 (2.252)	15.999 (2.101)	16.065 (2.258)
Teacher-reported tenth-grade track						
Advanced/honors	-	0.121	-	0.204	-	0.071
Academic or general	-	0.879	-	0.796	-	0.743
Discrepant	-	-	-	-	-	0.186
Self-reported eighth-grade ability group						
High	0.344	-	0.281	-	0.197	-
Middle/low	0.481	-	0.431	-	0.322	-
Not grouped	0.175	-	0.288	-	0.166	-
Discrepant: High or middle/low	-	-	-	-	0.179	-
Discrepant: Other combination	-	-	-	-	0.136	-
<i>Family background controls</i>						
Intact family (ref.: non-intact)	0.696	0.668	0.696	0.666	0.701	0.670
Father employed (ref.: not employed)	0.921	0.879	0.929	0.880	0.920	0.878
Mother employed (ref.: not employed)	0.900	0.885	0.906	0.886	0.908	0.892
At least one parent has died (ref.: no parents have died)	0.031	0.041	0.033	0.044	0.031	0.039
<i>Adolescent-specific controls</i>						
Average test score (range 0-10)	4.147 (1.939)	5.289 (2.278)	4.172 (1.934)	5.305 (2.259)	4.253 (1.897)	5.407 (2.242)
Average grade (1: mostly Ds through 5: mostly As)	4.048 (0.766)	4.094 (0.798)	4.069 (0.761)	4.125 (0.778)	4.088 (0.748)	4.120 (0.773)

(Continued)

Table 1. continued

	Mathematics		English		Combined placement	
	Eighth grade (1988)	Tenth grade (1990)	Eighth grade (1988)	Tenth grade (1990)	Eighth grade (1988)	Tenth grade (1990)
Delinquency scale (range 0–10)	1.142 (1.109)	1.571 (1.094)	1.130 (1.079)	1.565 (1.101)	1.094 (1.050)	1.515 (1.061)
Self-esteem (range 0–10) ^a	5.206 (1.302)	5.081 (1.280)	5.208 (1.289)	5.113 (1.272)	5.216 (1.302)	5.123 (1.253)
Locus of control (range 0–10) ^b	6.747 (1.560)	6.627 (1.527)	6.781 (1.561)	6.672 (1.530)	6.807 (1.540)	6.707 (1.524)
Teacher assessment scale (range 0–10) ^c	8.981 (1.671)	9.174 (1.262)	9.011 (1.640)	9.152 (1.311)	9.103 (1.538)	9.209 (1.183)
Peer perception scale (range 0–10)	5.659 (2.180)	5.388 (2.105)	5.657 (2.128)	5.420 (2.100)	5.681 (2.157)	5.415 (2.075)
Parents expecting adolescent to graduate college (ref.: not graduate)	0.755	0.708	0.759	0.706	0.753	0.709
<i>School-level controls</i>						
School socioeconomic context scale (range 0–10)	7.402 (1.473)	6.944 (1.286)	7.378 (1.548)	6.875 (1.352)	7.407 (1.515)	6.999 (1.283)
Public school (ref. private)	0.879	0.898	0.874	0.896	0.883	0.896
More than 1/3 of adolescents in college-bound tracks	–	0.033	–	0.025	–	0.021
N (adolescents)	6,013		7,217		3,269	
N (eighth-grade school units)	655		749		458	
N (high school units)	779		874		528	

Note: Combined weight applied (weight provided in NELS multiplied with the inverse probability of being in the sample conditional on race, gender, and parental socioeconomic status).

^aRosenberg's self-esteem scale provided in NELS. ^bRotter's locus of control scale provided in NELS. ^cScale is the average of two subject-specific teacher reports on the behaviors of the adolescent.

Teacher-Reported Track Placement in the Tenth Grade

I use the teacher-reported course level in tenth-grade mathematics and English for constructing indicators of track placement. In NELS, the tenth-grade teacher is asked to categorize the adolescent's class in one of five tracks: (1) advanced or honors, (2) academic, (3) general, (4) vocational, or (5) other. I restrict my analysis to the three first tracks, assuming that they form a general ranking of tracks in terms of content, level, and rigor, with advanced/honors being high rank; academic, middle rank; and general, low rank. Catsambis (1994) notes that for mathematics, this ranking reflects the actual curricular differentiation, as measured by teachers' reports on the topics covered in class. Gamoran and Carbonaro (2003) report a similar pattern for English. Moreover, because teachers represent the institutional authority of the school, using teacher reports might add to the potency of the track labels.

In the empirical analysis, I examine the consequences of placements in each subject and, combining placements in the two subjects, I investigate the effects of consistent versus discrepant track placements in the overall tracking structure. To make these two parts of my analysis compatible and to simplify exposition, I collapse adolescents placed in academic or general classes into one group.⁶ Consequently, this crude track indicator differentiates at the top of the track distribution, separating elite- or high-track adolescents from middle- or low-track adolescents. Given the paper's focus on the signaling value of track placement, using this crude indicator suffices for testing its theoretical predictions, and additional analyses (not reported here) also show that not collapsing tracks reveals the same basic pattern of findings that I report in the empirical analyses.

In the analyses combining course placements in mathematics and English, I construct an overall track indicator, which divides adolescents into three overall groups: those who are consistently placed in advanced/honors classes, those who are consistently placed in general or academic classes, and those who have discrepant course placements (defined as those being placed in an advanced/honors class in one subject and in a general or academic class in the other).

Self-Reported Ability Group in the Eighth Grade

In NELS, adolescents report whether their ability group in eighth-grade mathematics and English is high, middle, or low, or whether they are grouped at all. I use this information to measure an adolescent's tracking experience prior to high school enrollment. Dauber, Alexander, and Entwisle (1996) found that ability-group placement is stable from sixth to eighth grade, suggesting that the eighth-grade ability group is a good proxy for previous ability-group experience. Given the small fraction reporting being in a low-ability group, I collapse the middle and low groups. As was the case for the tenth-grade track placement, I construct an indicator of overall ability-group placement in eighth grade. This indicator groups adolescents into those who are consistently grouped as either high or middle/low track, those who are consistently not grouped, those who have discrepant course-level placements in mathematics and English, and those who have less clear discrepant placements (i.e., those who are ungrouped in one subject but grouped in the other).

Movements in the Stratified Curriculum from Middle to High School

In the empirical analysis, I combine the eighth- and tenth-grade curricular position indicators to define groups of movers and stayers in the stratified curriculum from middle to high school. Although I explain these groups more fully in the empirical analysis (see tables 4 and 5 for the empirical translations I use), I emphasize that the two indicators are not fully compatible and cannot, for institutional reasons, be made so. This partial compatibility introduces some indeterminacy into the definitions of movers and stayers in my analysis. Nevertheless, as I later explain, combining the two indicators still allows for meaningful comparisons required for testing the theoretical stipulations central to this paper. Moreover, because not all adolescents are ability grouped in eighth grade, I can also evaluate the effects of high school track placements among adolescents with no previous tracking experience. This property of the data provides me with the opportunity to evaluate educational tracking impacts that are insensitive to the partial compatibility of curricular positions.

Control Variables

To interpret track placement as a signal about academic abilities, I adjust my estimations for the range of time-varying control variables described in table 1. First, I control for exogenous changes in the family situation of the adolescent, changes that may jointly affect track allocation and the development of educational expectations. Second, to control for the learning mechanism described earlier, I include averaged test scores in math and reading and averaged grades (i.e., grade point average, GPA) in math and English in the estimations. Including the GPA also allows me to compare the magnitudes of track placement effects to those of the GPA effects. Third, to control for the influence of parents in expectation formation—a central stipulation in the status attainment tradition (Haller 1982; Sewell, Haller, and Portes 1969)—I include a self-report on whether at least one of the adolescent's parents expects the adolescent to complete college.

Fourth, to control for changing teacher influences (Kelly and Carbonaro 2012), I include a scale based on teacher assessments of whether the adolescent is frequently absent, tardy, inattentive, disruptive, or rarely completes his or her homework. Fifth, because the transition into high school—and, within high schools, into tracks—often alters an adolescent's social relationships (Crosnoe 2002; Kubitschek and Hallinan 1998), I include variables that either directly or indirectly control for this peer effects mechanism. These variables measure changes in delinquent behavior (using appropriate self-reports), self-esteem, locus of control, peer perceptions (using self-reports on how classmates view the adolescent), the socioeconomic context of schools (using various indicators on the school's socioeconomic composition), the school type, and whether more than one-third of the adolescents in the high school are in college-bound tracks.⁷

Analytical Approach

The aim of my empirical analysis is to examine the role of adaptation in adolescents' expectation formation. To meet this aim, I use a difference-in-differences

(DID) approach to isolate the signaling effects of high school track placement on educational expectations. This approach allows me to control for all unmeasured characteristics of adolescents that do not change over time (Halaby 2004; Gangl 2010). The approach is therefore an improvement over that used in previous studies, which typically control for measures of academic ability, family background, and expectations at the onset of high school (Alexander and Cook 1982; Wiatrowski et al. 1982; Vanfossen, Jones, and Spade 1987). Indeed, because expectations measured at the onset of high school may be a consequence of initial track placement, the approach used in previous studies will likely fail to control for selectivity bias (Rosenbaum 1984). This issue is particularly pertinent in the study of expectation formation, because adolescents “adapt their personal qualities in anticipation, even prior to attendance” (Meyer 1977, 63).

The DID approach I adopt compares the average difference in expectations between tracks *before* adolescents enter high school, in eighth grade (d_1), to the average difference *in* high school, in tenth grade (d_2). The difference between these two differences is the DID estimate of the track effect, $\delta = d_2 - d_1$, and is mathematically equivalent to the between-track difference in the average change in expectations from middle to high school. As Halaby (2004) demonstrates, the DID approach is a member of the class of panel fixed-effects methods that exploits over-time changes in variables to estimate causal effects. Thus, the DID estimates reported in this paper are based on the first-differences estimator, which in a balanced two-period panel yields the DID estimates (Angrist and Pischke 2009).⁸ Adopting this specification also allows me to control the DID estimates for time-varying control variables.

The DID estimate can be given a causal interpretation under the assumption that, in the absence of tracking, the average difference in expectations between adolescents in two tracks would be the same over time (Angrist and Pischke 2009). To maintain this identifying assumption, I extend the DID approach in two ways. First, because various individual and institutional factors are likely to change as adolescents move from middle to high school (e.g., Lord, Eccles, and McCarthy 1994), any comparison of changes in expectations between tracks may pick up these other changes—changes that do not reflect the signaling effect of interest to this paper. To deal with this problem and thereby isolate the signaling effect, I control for the time-varying variables previously described.

Second, because inference based on DID rests on modeling how adolescents' expectations would have changed, had they not been in a particular track in high school, picking proper control groups to form these counterfactual changes becomes important to any proper evaluation of the effects of track placement on educational expectations. To deal with this problem, I exploit the rich information on ability-grouping experiences in eighth grade to compare adolescents that differ by the extent to which their high school track placement can be said to reveal new information to them. The guiding idea is to compare changes in educational expectations from middle to high school among groups of movers and stayers constructed from adolescents' positions in the stratified curriculum in both middle and high school.⁹ In this setup, the change in expectations experienced by stayers is taken to be the change that movers would have experienced, had they remained in their initial curricular position. Put differently, this extended

approach compares adolescents who occupy similar positions in the stratified curriculum in eighth grade but who differ in terms of their positions in tenth grade.

Despite the advantages of the DID approach to evaluating the role of educational tracking in expectation formation processes, my application is vulnerable to two limitations set by the data. First, the collection of observations in NELS is separated by two years, from eighth to tenth grade. Changing expectations in ninth grade could therefore potentially cause a change in track placement in tenth grade, leading to upwardly biased track effects. However, because adolescents have only partial control over their course-level placements, I consider this scenario as having negligible influence on the substantive results I report here.¹⁰

Second, the curricular positions in eighth and tenth grade are, for reasons explained earlier, not fully compatible. Nevertheless, my analyses still provide sufficient grounds for meaningfully separating adolescents according to the extent to which track placement in high school conveys new information to them. For example, if adolescents moving from a comparatively low curricular position in middle school to a comparatively high position in high school regulate their expectations to a degree larger than do adolescents staying in comparatively low positions, I take this finding as supporting the contention that adolescents adapt their expectations to the ability signals sent by their track placement. Moreover, exploiting the fact that not all adolescents are ability grouped in eighth grade, I can estimate effects that are insensitive to any incompatibility of curricular positions. This latter group therefore provides me with a powerful control group that enhances my inferences about the role of educational tracking in adolescents' expectation formation.

Results

Placement Effects on Educational Expectations in Mathematics and English

Does track placement in tenth-grade mathematics and English affect educational expectations, or are differences already in place before adolescents enter high school? To answer this question and to give an exposition of the logic of the DID approach, I first report results that disregard adolescents' ability-group placements in eighth grade. Thus, in table 2, I report DID estimates of the effect of tenth-grade track placement, estimates that involve comparing, for each subject, the average change in expectations (from eighth to tenth grade) between the two tenth-grade tracks. In both subjects, the change in expectations of advanced/honors course takers is noticeably larger than the change of academic or general course takers (panel A). The difference between the two changes, that is, the gross DID estimate, is of about 0.4 years of expected education in each subject. Although controlling the estimates for other factors that might change during the transition to high school results in minor reductions, the controlled, direct effects remain substantial, statistically significant, and in the predicted directions (panel B). Therefore, adolescents appear to regulate their educational expectations in light of the signals sent by their high school track placements.

Nevertheless, the results reported in table 2 ignore the possibility that adolescents differ by the extent to which track placement in high school reveals new information to them about their academic abilities. For some adolescents, their position in the stratified high school curriculum will be similar to the curricular position they held in middle school; for others, the positions will differ. To investigate whether movers in the stratified curriculum regulate their expectations more than stayers do (from eighth to tenth grade), I report in table 3 the DID estimates based on the empirical translation of stayers and movers provided in table 4. Although the estimates differ somewhat between mathematics and English, the overall pattern of effects reported in table 3 shows that adolescents respond in the predicted directions to the signals sent by their high school track placements.

For example, in mathematics, “low- to high-track movers” (i.e., moving from a middle or low eighth-grade ability group to a tenth-grade advanced/honors class) upwardly regulate their expectations by about half a year more than do “low-track stayers” (i.e., moving from a middle or low eighth-grade ability group to a tenth-grade academic or general class). This estimate is statistically significant, in the predicted direction, and robust to changing the reference group of stayers from low-track stayers to “all stayers,” thereby showing the significant impact of high-track placements in high school mathematics.¹¹ Although the corresponding estimate for English is about one-third of a year of expected

Table 2. Difference-in-Differences (DID) Estimates of the Effect of Tenth-Grade Track Placement on Educational Expectations in Mathematics and English

	Mathematics course placement			English course placement		
<i>Panel A: Gross levels and differences in expectations</i>						
	Eighth grade	Tenth grade	Difference	Eighth grade	Tenth grade	Difference
Advanced/honors class	17.058	17.424	0.367	16.937	17.264	0.326
Academic or general class	15.825	15.771	−0.054	15.750	15.678	−0.073
Difference	1.233	1.653	0.421	1.187	1.586	0.399
<i>Panel B: DID estimates (advanced/honors – academic or general)</i>						
DID (no controls)		0.421*** (0.091)			0.399*** (0.085)	
DID (family background controls)		0.416*** (0.091)			0.394*** (0.085)	
DID (all controls)		0.377*** (0.090)			0.355*** (0.080)	

Note: Weight used (see note in table 1). Sample design corrected standard errors. Numbers may deviate slightly because of rounding. Years of expected education. Standard errors in parentheses.

*** $p < .001$

Table 3. Difference-in-Differences Estimates of High School Track Placement Effects on Educational Expectations, Based on Constructed Groups of Stayers and Movers in the Math and English Tracking Structure from the Eighth to Tenth Grade (see table 4 for the empirical translation of these groups)

	Mathematics			English		
	No controls	Family background controls	All controls	No controls	Family background controls	All controls
<i>Downward movements:</i>						
High- to low-track movers [II]	-0.279* (0.123)	-0.271* (0.123)	-0.163 (0.116)	-0.434*** (0.127)	-0.425*** (0.126)	-0.399** (0.119)
High- to low-track movers [II]	0.046 (0.095)	0.061 (0.095)	0.013 (0.098)	-0.128 (0.099)	-0.091 (0.100)	-0.152 (0.100)
<i>Upward movements:</i>						
Low- to high-track movers [III]	0.590** (0.205)	0.575** (0.205)	0.526** (0.211)	0.366† (0.202)	0.374† (0.201)	0.375† (0.199)
Low- to high-track movers [III]	0.530** (0.203)	0.512* (0.204)	0.504* (0.212)	0.280 (0.200)	0.277 (0.201)	0.340† (0.198)
<i>From non-tracked to tracked system:</i>						
Ungrouped to high-track movers [V]	0.653* (0.276)	0.667* (0.278)	0.778** (0.288)	0.423* (0.175)	0.418* (0.176)	0.304* (0.153)

Note: Weight used (see note in table 1). Sample design corrected standard errors. Years of expected education. Standard errors in parentheses. *** $p < .001$ ** $p < .01$ * $p < .05$ † $p < .10$

Table 4. Empirical Translation of Movers and Stayers in the Mathematics or English Tracking Structure from the Eighth to Tenth Grade, Defined by the Eighth-Grade Ability-Group Indicator and Tenth-Grade Track Placement Indicator

	Tenth-grade track placement	
	Advanced/honors	Academic or general
Eighth-grade ability-group placement:		
High	<i>High-track stayers [I]</i>	<i>High- to low-track movers [II]</i>
Middle/low	<i>Low- to high-track movers [III]</i>	<i>Low-track stayers [IV]</i>
Not grouped	<i>Ungrouped to high-track movers [V]</i>	<i>Ungrouped to low-track movers [VI]</i>

Note: Unweighted group sizes for Mathematics are [I] 600; [II] 1,545; [III] 98; [IV] 2,734; [V] 123; and [VI] 914.

Unweighted group sizes for English are [I] 824; [II] 1,240; [III] 346; [IV] 2,773; [V] 294; and [VI] 1740.

education, this estimate is not statistically significant at a 5 percent level (although the p -value is less than 0.1).

In contrast, table 3 shows a less clear picture for downward movers in the stratified curriculum. Although some of these effects are statistically significant and in the predicted directions, they are not robust to using the pooled group of all stayers as the reference group for those not changing tracks. This lack of symmetry in the effects of downward and upward movements suggests that positive ability signals loom larger than negative ones in adolescents' expectation formation. However, this configuration of effects may, as explained earlier, result from the partial compatibility of the eighth- and tenth-grade curricular position indicators. This partial compatibility makes substantively interpreting the pattern of effects difficult. Nonetheless, detecting any heterogeneity in responses is sufficient for testing the theoretical predictions central to this paper. Therefore, I take the significant impact of upward movement in the tracking structure as supporting the contention that high school track placement offers new information to which adolescents respond.

This finding is further corroborated by the DID estimates reported in table 3 for adolescents who were not ability grouped in eighth grade. Because track effects for this group of adolescents are insensitive to the particular empirical translation of stayers and movers in table 4, they provide a strong test of the informational effects of high school track placement. Controlling for all variables, the estimates show that this group of adolescents respond in the predicted direction to their track placement in both subjects. Yet, the response appears more substantial for placement in mathematics (about three-quarters of a year) than for placement in English (about one-third of a year). This difference between the two subjects may arise because advancement in mathematics is of a comparatively more cumulative and differentiated nature (Gamoran 1987). According to this view, the differentiation of the math curriculum may convey a clearer signal about an adolescent's relative standing than the corresponding differentiation in the English curriculum. However, despite this difference, on balance the subject-specific analyses show significant informational effects of high school track placement in both mathematics and English.

Effects of Consistent and Discrepant Track Placements

The key conjecture of this paper is that track placement serves as a structural location that systematically conveys information to the adolescent. To investigate whether adolescents with consistent course-taking patterns receive a stronger signal regarding their academic abilities than adolescents with discrepant course-taking patterns, I compare changes in expectations from middle to high school among groups of stayers and movers in the overall tracking structure. I provide the empirical translation of these groups in table 5.¹² In contrast to the subject-specific movements analyzed earlier, factoring in discrepant placements allows me to differentiate *within* groups of movers and stayers. I can distinguish between those moving between consistent track placements (e.g., from consistent placements in middle- or low-ability groups in eighth grade to consistent placements in advanced/honors classes in tenth grade) and those who make partial moves (e.g., from consistent placements in high-ability groups in eighth grade to discrepant course placements in tenth grade). Moreover, I can distinguish between “consistent track stayers,” who keep their consistent placements, and “discrepant track stayers,” who keep their particular combination of course levels in the two subjects. This distinction provides two control groups of adolescents for whom track placement in high school arguably conveys little new information.

Table 6 reports track effects based on the grouping of movers and stayers in table 5. The cells show DID estimates comparing changes in expectations (from eighth to tenth grade) between movers and stayers, defined respectively in the rows and columns of the table. The table reveals two findings. First, whereas “consistent high- to low-track movers” appear to regulate their educational expectations to a degree much similar to the various control groups of stayers, the corresponding impact of about 1.5 years of expected education among “consistent low- to high-track movers” is positive, statistically significant, and very substantial (panel A). This lack of symmetry in the effects of upward and downward movements in the consistent tracking structure appears to corroborate the finding reported in the subject-specific analyses, namely, that positive ability signals loom larger than negative ones in adolescents’ formation of expectations. However, as explained earlier, this pattern of effects may simply result from the partial compatibility of the eighth- and tenth-grade curricular position indicators. This partial compatibility makes it difficult to draw any firm conclusions about the pattern. Nonetheless, as explained earlier, the pattern of effects still provides sufficient support for the theoretical predictions. Thus, in line with these predictions, the informational effect of being consistently placed in high-track classes in high school is particularly substantial among those who, given their consistent low-track placements in eighth grade, are those least expected to enter this position in the stratified high school curriculum.

Second, table 6 shows that partial movers in the tracking structure do not revise their educational expectations to a degree that is statistically significantly different from the various control groups of stayers (panel B). In other words, there is little signaling impact of moving from consistent placements in middle school to discrepant placements in high school, or of moving from discrepant placements in middle school to consistent placements in high school (when compared to those

Table 5. Empirical Translation of Groups of Stayers and Movers in Tracking Structure from the Eighth to Tenth Grade, Defined by the Combined Eighth-Grade Ability-Group Indicator and the Combined Tenth-Grade Track Placement Indicator

Combined tenth-grade track placement			
Consistent placement		Discrepant placement	
Advanced/honors		Advanced/honors: Math Academic or general: English	Advanced/honors: English Academic or general: Math
Consistent eighth-grade ability-group placement:			
High	Consistent high-track stayers [A]	Consistent high- to low-track movers [B]	Partially downward movers [C] Partially downward movers [D]
Middle/low	Consistent low- to high-track movers [E]	Consistent low-track stayers [F]	Partially upward movers [G] Partially upward movers [H]
Not grouped	Consistently ungrouped to consistent high-track movers [I]	Consistently ungrouped to consistent low-track movers [J]	Consistently ungrouped to discrepant track placement movers [K] Consistently ungrouped to discrepant track placement movers [L]
Discrepant eighth-grade ability-group placement:			
High: Math Middle/low: English	Partially upward movers [M]	Partially downward movers [N]	Discrepant track stayers [O] -
High: English Middle/low: Math	Partially upward movers [P]	Partially downward movers [Q]	- Discrepant track stayers [R]
Other discrepant placement	-	-	-

Note: Unweighted group sizes are [A] 147; [B] 268; [C] 62; [D] 160; [E] 8; [F] 899; [G] 34; [H] 66; [I] 17; [J] 427; [K] 29; [L] 35; [M] 29; [N] 257; [O] 36; [P] 8; [Q] 188; [R] 66. The number of unweighted respondents in the remaining (excluded) groups is 514.

Table 6. Difference-in-Difference Estimates of the Effect on Educational Expectations of Combined High School Track Placement, Based on Constructed Groups of Stayers and Movers in the Tracking Structure from the Eighth to Tenth Grade (see table 5 for the empirical translation of these groups)

	Groups of stayers			
	Consistent high-track stayers [A]	Consistent low-track stayers [F]	Consistent track stayers pooled [A + F]	Discrepant track stayers pooled [O + R]
<i>Panel A: Movers in consistent tracking structure</i>				
Consistent high- to low-track movers [B]	-0.323 (0.217)	-	0.076 (0.172)	0.144 (0.296)
Consistent low- to high-track movers [E]	-	1.680** (0.596)	1.672** (0.606)	1.740** (0.663)
<i>Panel B: Partial movers in tracking structure</i>				
Partially downward movers pooled [C + D + N + Q]	-	-	0.038 (0.143)	0.106 (0.682)
Partially upward movers pooled [G + H + M + P]	-	-	0.115 (0.213)	0.183 (0.313)

Note: Weight used (see note in table 1). Sample design corrected standard errors. Table cells contain DID estimates that subtract the change in expectations among stayers (in the columns) from the change in expectations among movers (in the rows). Controlling for all control variables. Years of expected education. Standard errors in parentheses.
***p* < .01

who keep either their consistent or their discrepant placements from middle to high school). Analyses (not reported here) that break down the types of partial movers (according to the possible movements defined in table 5) corroborate this finding of no signaling effects for these groups of adolescents. Consequently, the results in table 6 support the contention that consistent course placements convey a clearer signal to the adolescent than discrepant course placements do.

However, drawing this conclusion may depend on the compatibility of the eighth- and tenth-grade curricular position indicators. Thus, I report in table 7 the DID estimates for adolescents who are consistently not ability grouped in eighth grade. These adolescents have little experience with any curricular differentiation prior to high school, and—as was the case for the subject-specific analyses—the estimates are therefore insensitive to the particular empirical translation of stayers and movers (see table 5). Table 7 shows that, for this group of adolescents—compared to consistent low-track placement (i.e., in two academic or general classes)—consistent high-track placement (i.e., in two advanced/honors classes) significantly boosts expectations by about one year, whereas discrepant track placement significantly increases expectations by about half a year (panel A). However, although the difference between these two estimates supports the contention that consistent track placement provides a clearer signal to adolescents than discrepant placement does, the difference—which is the difference between consistent high-track placement and discrepant track placement—is not

Table 7. Difference-in-Differences (DID) Estimates of the Impact of Consistent and Discrepant High School Track Placement on Educational Expectations among Adolescents Consistently Not Ability-Grouped in the Eighth Grade (see table 5 for the empirical translation of groups)

	DID (All controls)
<i>Panel A: DID estimates</i>	
Consistently ungrouped to consistent high-track movers [I]	0.963 [†]
– Consistently ungrouped to consistent low-track movers [J]	(0.555)
Consistently ungrouped to discrepant track placement movers pooled [K + L]	0.585*
– Consistently ungrouped to consistent low-track movers [J]	(0.274)
Consistently ungrouped to consistent high-track movers [I]	0.378
– Consistently ungrouped to discrepant track placement movers pooled [K + L]	(0.606)
<i>Panel B: DID estimates based on collapsing consistent and discrepant course takers</i>	
Being in at least one advanced/honors class [I + K + L]	0.665**
– Being in no advanced/honors class [J]	(0.255)

Note: Weight used (see note in table 1). Sample design corrected standard errors. Controlling for all control variables. Years of expected education. Standard errors in parenthesis.

** $p < .01$ * $p < .05$ [†] $p < .10$

statistically significant at conventional levels. Thus, discrepant placement appears to convey as clear a signal as consistent placement. Put differently (and illustrated in panel B in table 7), among adolescents with little prior tracking experience, being in at least one advanced/honors class in high school, compared to being in none, significantly affects educational expectations.

Taken together, the analyses of combined track placements provide mixed support for the conjecture that adolescents revise their educational expectations more strongly to consistent, as opposed to discrepant, course-taking patterns. Nonetheless, the analyses show clear signaling effects of being in advanced/honors classes in high school, particularly for adolescents for whom their track placement—given their prior tracking experience or inexperience—can be said to convey new information to them about their academic potential and consequent chances for success in future schooling.

Interpreting Track Effects

The findings in my analysis suggest that adolescents revise their educational expectations consistently and in the predicted directions in light of the signals that their high school track placements send. Yet, the question remains of how large these effects are. Answering this question is important for analyzing the role of adaptation in adolescents' formation of educational expectations. To interpret the magnitudes of the track effects, I use three approaches. The first approach exploits the panel data and examines the extent to which observed differences in expectations between tracks in high school were in place before adolescents enter high school. Given that much of the literature on expectation formation focuses

on ability signals sent by the GPA, the second approach compares the magnitude of track effects to that of the effect of the GPA. The third approach compares the magnitude of track effects to average differences in expectations across background variables known to correlate with educational attainment.

For the first approach, I break down the total difference in educational expectations in tenth grade into a component attributable to the difference that existed in eighth grade and a component attributable to the effect of the tenth-grade track placement. This approach therefore gives the relative importance of track placement vis-à-vis the selection into tracks on stable, unobserved characteristics in the formation of educational expectations. Analyses (not reported here) show that roughly 50 percent of the total tenth-grade difference in expectations between consistent low- to high-track movers and consistent low-track stayers is attributable to the effect of track placement. Thus, for this comparison, the track effect is as important as the selection effect in bringing about track differences in educational expectations in high school. Decomposing the tenth-grade difference in expectations between consistent placement in advanced/honors classes and consistent placement in academic or general classes among adolescents not ability grouped in eighth grade yields a corresponding percentage of 30. Thus, these results suggest that although high school track differences in expectations are in place before adolescents enter high school, tracking plays an important role in conveying pertinent information in light of which adolescents form their educational expectations.

For the second approach, I compare the track effects with the GPA effect on expectations. Estimations (not reported here) suggest that the GPA effect is about 0.33 years of expected education (a result found in all subsamples used in this paper). An adolescent would thus have to move about 1.5 units along the GPA scale (e.g., from a C average to a B+ average)—a considerable achievement—before adapting his or her educational expectations to an extent that parallels the adaptation reported in table 3 for low- to high-track movers (compared to low-track stayers) in mathematics. Moreover, the adaptation to track placement among consistent low- to high-track movers (compared to consistent low-track stayers) reported in table 6 would correspond to a move across the entire GPA distribution. In this respect, track placement appears to strongly influence adolescents' educational expectations.

For the third approach, I compare the effects of high school track placement on educational expectations to the influence of two variables known to affect educational attainment: parental income and ability.¹³ Estimations (not reported here) show that the weighted average difference in eighth-grade expectations across adjacent quartiles in the distribution of parental income is about half a year of expected education, whereas the corresponding difference across test scores in mathematics and reading in eighth grade is about three-quarters of a year. Thus, the effect of consistent low- to high-track movers (compared to consistent low-track stayers) corresponds to three times the average difference in expectations between two income quartiles and twice the difference in expectations between two ability quartiles. Given the impact of these variables on educational attainment, this finding suggests that tracking in high school exerts a strong influence on educational expectations.

Discussion and Conclusion

This paper examines the role of high school track placement in the formation of adolescents' educational expectations and makes three contributions to the literature. First, I use educational tracking to evaluate the extent to which adolescents adapt their educational expectations to new information that helps them determine their chances for success in future schooling. Second, I argue that any proper evaluation of the informational effects of high school track placement requires comparing adolescents that differ by the extent to which their placement reveals new information to them about their academic abilities. Third, applying a DID approach that controls for the non-random selection into tracks in high school, I isolate the signaling effect of high school track placement on educational expectations.

The empirical analysis provides evidence in favor of the theoretical position stressing the crucial role of information in adolescents' formation of beliefs about their future. I find that adolescents actively revise their educational expectations in response to their track placements in high school—an ability signal whose value, I argue, derives from its relation to adolescents' perceived chances for success in future schooling. As expected, these revisions are particularly pronounced when placement is consistent across subjects, and they exist primarily when placements in high school contradict tracking experiences in middle school. In terms of magnitude, the effects of track placement are substantial when considered in relation to both expectation formation processes that occur before high school and the impact on expectations of variables known to be strongly linked to educational attainment.

My study has four implications for research on the formation of adolescent educational expectations. First, the informational effects of the sorting of students according to academic abilities are likely to differ among adolescents. The findings illustrate that educational expectations are formed in the interaction between individual biographies and institutionally defined expectations. Prior experience in the educational system shapes the conditions under which adolescents respond to the signals sent by schools. Thus, future research on expectation formation among adolescents should examine how these conditions evolve and function, and should develop taxonomies for educational trajectories that can provide a backdrop for interpreting the effects of ability sorting on educational expectations.

Second, while this paper shows that educational tracking plays a role in the formation of adolescents' educational expectations, it says little about the evolution of educational expectations before adolescence. As Andrew and Hauser (2011) note, we lack a consistent theory of the development of educational expectations. In status attainment theory, significant others respond to the signals conveyed from grades in the expectations these others hold for the individual adolescent (e.g., Haller and Portes 1973). Throughout elementary school, parents receive signals about the academic abilities of their children and likely use this information to form their expectations for them. In this way, the educational expectations that individuals hold at the onset of adolescence are the result of socializing influences, which reflect the learning processes of their parents.

Understanding and analyzing these dynamic processes requires not only a theoretical framework that views the evolution of beliefs as the outcome of joint learning processes in the family, but also rich panel data on beliefs, expectations, and outcomes related to academic success over the educational careers of individuals.

Third, research on expectation formation needs better measures of adolescents' educational expectations. The measure I use does not provide information on the certainty with which an adolescent expects a given level of educational attainment. Recent work on the elicitation of expectations conceives of expectations as subjective probability distributions (Manski 2004). In this view, each adolescent assigns a probability to each potential level of education, yielding an individual-specific distribution of potential years of education. Eliciting these expectation distributions would allow for modeling both the level and dispersion of these distributions, thereby providing better insights into the functions of educational tracking in the formation of educational expectations. However, to study these critical aspects of expectation formation, future research needs to collect fine-grained data on expectation distributions.

Fourth, my study demonstrates that educational tracking stratifies conceptions of academic ability, conceptions through which adolescents come to view themselves and to which they respond in terms of modifying their educational expectations. Because these labeling processes operate independent of actual abilities, they tend to produce self-fulfilling prophecies (Merton 1948). Labeled a high-track or low-track student, an adolescent is likely to change goal orientation, in turn possibly leading to behavioral changes that will tend to conform to these labels. Given the strong impact of educational expectations on final educational attainment, this feedback mechanism has two related consequences for inequalities in educational attainment. On the one hand, the mechanism is likely to perpetuate inequalities in educational attainment. On the other hand, because track placement correlates with socioeconomic background, tracking in high schools is likely to reinforce preexisting socioeconomic inequalities in educational expectations and consequent educational attainment. Either way, the educational system ends up serving a purpose—that of stratifying adolescents, independent of their academic potential—very different from its intentions.

Notes

1. Much of this literature shows that adolescents often have imperfect knowledge about the objective aspects of future schooling, such as the costs of college (Grodsky and Jones 2007), the returns to schooling (Dominitz and Manski 1996), the academic demands of higher education (Rosenbaum 1998), and the educational requirements of jobs (Morgan et al. 2013). In light of this literature, it also appears likely that adolescents use new information to resolve some of the uncertainties associated with these aspects of future schooling. I return to the issue of uncertain beliefs in the concluding section of the paper.
2. Morgan's (1998) analysis of historical trends in high school seniors' educational expectations presents related evidence that adolescents respond to information relevant to educational decision-making. Morgan found that trends in expectations

mirror changes in the earning returns to education, suggesting that adolescents respond to the changing costs and benefits associated with educational decisions (see also Wilson, Wolfe, and Haveman 2005). Similarly, studies have long documented the increasing realism of students' educational expectations over the educational career (Bozick et al. 2010; Kerckhoff 1977), reflecting both the development of cognitive self-appraisals and a process of self-reflexive adaptation to changing environments (Stipek and Mac Iver 1989; Wigfield and Eccles 2000).

3. Factors beyond the control of adolescents relate not only to academic potential and teacher decisions but also to between-school differences in the implementation of tracking (Lucas and Berends 2002; Kelly 2007). Even among adolescents with similar abilities and family background, opportunities to enter high-track classes may vary idiosyncratically between schools and, within schools, between subjects—thus making placements in the stratified curriculum beyond the immediate control of adolescents (and their families).
4. To maximize efficiency, I use the imputation strategy that deletes missing values on the outcome variable after imputation (Von Hippel 2007). I estimate the inverse probability weight conditional on gender, race, and parental socioeconomic status. Results based on listwise deletion and no probability weights, albeit less efficient, are very similar to those reported here, indicating the robustness of my findings to alternative specifications.
5. Following previous research (e.g., Hallinan and Kubitschek 1999), the final samples exclude high school dropouts and adolescents who never attended high school. I further omit adolescents classified as taking a vocational course, because this group constitutes only a minor fraction of the samples and because the track indicator I use differentiates among ordered tracks, thereby allowing me to make comparisons necessary for testing the claims central to this paper.
6. Simplifying exposition also becomes important, because, as I later explain, a substantial part of the empirical analysis combines eighth-grade ability grouping and tenth-grade tracking, yielding a plethora of possible combinations of course-level movements from middle to high school.
7. Information on the construction of these variables is available from the author upon request. As the last school characteristic is not available for the eighth-grade wave, it is not truly time-varying. To control for it in my first-difference estimations (described later), I therefore set its values to zero in the eighth-grade period.
8. The first-differences estimator I use effectively regresses changes in expectations on groups of adolescents defined by their track placements. For a formal exposition of the equivalence of these panel model estimators, Halaby (2004, 514–15).
9. This analysis is inspired by the strategy used by Meghir and Palme (2005) in their study of educational reform impacts in Sweden. I thank an anonymous reviewer for pointing out the relevance of this approach to my analyses.
10. Dauber, Alexander, and Entwisle's (1996) study corroborates this assumption. They report negligible effects of prior educational expectations on track placement in both the sixth and eighth grades, when controlling for prior performance and family background.
11. Using "all stayers" as an alternative reference group is inspired by Meghir and Palme (2005) and serves the simple purpose of constructing the counterfactual change in expectations using a more comprehensive group of adolescents for whom track placement conveys little new information.
12. Because table 5 has 24 possible combinations, I simplify the exposition by reporting only the groups of stayers and movers that I use for testing the theoretical predictions.
13. I use the eighth-grade measure of parental income provided in NELS.

About the Author

Kristian Bernt Karlson holds a postdoc position in the Department of Sociology at the University of Copenhagen. His research interests lie within the areas of educational stratification, social mobility, and quantitative methods. Recent papers appear in *Sociological Methods & Research*, *Social Science Research*, and *European Sociological Review*.

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How Has Educational Expansion Shaped Social Mobility Trends in the United States?

Fabian T. Pfeffer, *University of Michigan*

Florian R. Hertel, *Universität Bremen*

This contribution provides a long-term assessment of intergenerational social mobility trends in the United States across the 20th and early 21st centuries and assesses the determinants of those trends. In particular, we study how educational expansion has contributed to the observed changes in mobility opportunities for men across cohorts. Drawing on recently developed decomposition methods, we empirically identify the contribution of each of the multiple channels through which changing rates of educational participation shape mobility trends. We find that a modest but gradual increase in social class mobility can nearly exclusively be ascribed to an interaction known as the compositional effect, according to which the direct influence of social class backgrounds on social class destinations is lower among the growing number of individuals attaining higher levels of education. This dominant role of the compositional effect is also due to the fact that, despite pronounced changes in the distribution of education, class inequality in education has remained stable while class returns to education have shown no consistent trend. Our analyses also provide a cautionary tale about mistaking increasing levels of social class mobility for a general trend toward more fluidity in the United States. The impact of parental education on son's educational and class attainment has grown or remained stable, respectively. Here, the compositional effect pertaining to the direct association between parental education and son's class attainment counteracts a long-term trend of increasing inequality in educational attainment tied to parents' education.

Introduction

The empirical study of intergenerational class mobility is generally regarded as one of the workhorses of sociological stratification research (Ganzeboom, Treiman, and Ultee 1991; Hout and DiPrete 2006). After a paucity of research on

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US class mobility trends for about a quarter century, we have seen a recent resurgence of work in this area (e.g., Beller and Hout 2006a; Beller 2009; Mitnik, Cumberworth, and Grusky 2013; Long and Ferrie 2013). These renewed attempts at describing the broader historical patterns of progress toward an open society come at an interesting time—on the heels of the most significant economic downturn since the Great Depression (Grusky, Western, and Wimer 2011; Danziger 2013), when worries about decreasing levels of opportunity in the United States are widespread (e.g., Duncan and Murnane 2011; Corak 2013).

Nevertheless, reliable descriptions of long-term trends in class mobility are largely elusive for the US case (unlike for most European countries; see Breen 2004), and remain hotly debated (Xie and Killewald 2013; Hout and Guest 2013). We also still lack a full understanding of the determinants of long-term trends in social mobility. In particular, we do not know how social mobility trends have been shaped by one of the main mediators of intergenerational mobility, education, and by the fundamental shifts in its distribution, educational expansion. Given that the United States has lost ground and surrendered its former leadership role in educational participation to other countries over the past three decades (Goldin and Katz 2008; Garfinkel, Rainwater, and Smeeding 2010), this channel of social mobility is of profound interest in examinations of historical trends in the United States.

This contribution aims to establish cohort trends in social mobility over the entire 20th century for men and to provide an estimate of the degree to which changes in educational attainment and opportunity contributed to these trends. Earlier research, reviewed below, has yielded partial evidence on the question of whether educational expansion contributes to social mobility. We provide an empirical assessment of this question that joins prior evidence and expands on research that has directly estimated the relationship between educational expansion and social mobility for other countries (Breen 2010) and for an earlier historical period in the United States (Rauscher 2013). We test and ultimately confirm hypotheses developed in earlier work about the specific channels through which educational expansion impacts social mobility (Hout 1988).

We begin by reviewing existing theory and evidence that speak to the questions addressed here and also argue for the benefits of expanding our view beyond that of inequalities in opportunities tied to parental class. The empirical analysis begins with a description of educational expansion and changes in the class structure over the past seven decades. Our main analyses then focus on cohort changes in social class mobility and the role of education—first as a descriptive assessment and then in a decomposition analysis to dissect the contribution of different mechanisms through which educational expansion has shaped those mobility trends. We then apply these analyses to a different specification of individuals' social origins, namely their parents' educational status instead of their parents' social class.

Theoretical Background and Prior Evidence

Trends in Social Class Fluidity

As discussed in more detail in our Methods section, we study trends in social mobility that are more precisely termed trends in “social fluidity” or “exchange

mobility”—that is, changes in the association of socio-economic origin and destination independent of shifts in the occupational structure. During the 1970s and 1980s, the particularly laborious field of sociological research on class mobility established a slow upward trend in this type of mobility in the United States over much of the 19th and 20th centuries (Featherman and Hauser 1978; Grusky 1986; Hout 1984, 1988; DiPrete and Grusky 1990). The validity of recent contrary findings by Long and Ferrie (2013) has been challenged based on data limitations and modeling idiosyncrasies, including those lending undue influence to occupational mobility in the farming sector (Xie and Killewald 2013; Hout and Guest 2013). The general tendency of increasing US social class fluidity among men thus remains a valid description of the trend during the century leading up to the 1980s—a finding that also coincides with evidence for most other Western industrialized countries during this period (Breen 2004).

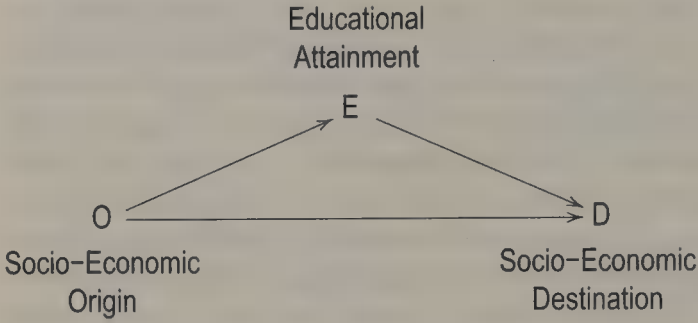
Recent research on trends since the mid-1980s has yielded some evidence that this trend toward increasing social class fluidity may have stalled or, in select dimensions of the class structure, even reversed. Although Beller (2009) finds a general pattern of stability in mobility chances, she shows variance in recent trends depending on whether and how maternal social class status is taken into account. Specifically, she finds a significant decline in social class fluidity for the cohort born 1965–1979, based on a specification of class background that includes a homemaker category for mothers that is further differentiated by educational status (Beller 2009, 521–23). Another recent contribution by Mitnik, Cumberworth, and Grusky (2013) analyzes period changes in social class fluidity and finds some evidence for an increase in the intergenerational association within the managerial/professional class.

The Effects of Educational Expansion

Partial Effects

The lack of reliable estimates of long-term social fluidity trends is mainly due to prior data limitations and changing occupational coding schemes (see the Data section). The comprehensive identification of factors that account for social fluidity trends, on the other hand, has been limited chiefly by conceptual and methodological problems. Figure 1 depicts the standard view of the socio-economic attainment process, in which socio-economic origin (e.g., parents' social class) exerts direct influences on socio-economic destination (e.g., children's social class) as well as indirect effects through offspring's educational attainment (Blau and Duncan 1967). The basic challenge we face in explaining trends in the total association between social origins and destinations, or social fluidity trends, is that each of these pathways—the direct transmission of socio-economic status across generations (OD association), the extent of socio-economic inequality in educational attainment (OE), and the socio-economic returns to educational degrees (ED)—may be subject to change over time. Furthermore, as we discuss below, interactions among the three variables may further complicate the assessment of overall fluidity trends, such as when the direct transmission of status differs by levels of educational attainment.

Figure 1. The mobility triad



Most of the above associations have been subject to extensive empirical study and—in the context of fundamental changes in the distribution of education—to much debate. Here, we briefly review some of the main theoretical approaches and findings on trends in selected parts of the mobility triad for the United States,¹ keeping in mind that they alone do not allow us to draw firm conclusions about the determinants of social fluidity trends. Also, while there are manifold theoretical propositions on the mechanisms behind each of the associations studied here, we limit our review to just the main theories concerned with temporal change in these associations. Finally, we focus on evidence involving measures of social class as indicators of socio-economic standing to inform our own analyses that draw on this conceptualization of social inequality and fluidity.

A theory devised explicitly to explain trends in educational inequality (OE) in the context of expanding educational participation is that of *Maximally Maintained Inequality* (Raftery and Hout 1993). MMI posits that massive educational expansion does not necessarily lower educational inequalities since privileged groups profit from it at higher rates. In this sense, MMI fits the aggregate pattern observed in the United States reasonably well, where massive educational expansion has had little effect on the level of social class inequality in educational attainment (Mare 1981, 1993; Hout, Raftery, and Bell 1993; Hout and Dohan 1996; Roksa et al. 2007). This is notably dissimilar to many European nations where class inequality in education has decreased with educational expansion (Breen and Jonsson 2005; Breen et al. 2009). In terms of the specific pattern of these trends, however, the US case (unlike some other countries; see Hout and DiPrete 2006) does not correspond well to theoretical predictions: MMI proposes that inequality at a given educational level decreases with expansion only when enrollment at that level is already saturated for the privileged classes. Inequality may then simply shift upward to the next educational level. The stability of educational inequality in the United States runs counter to both predictions. First, despite saturation of secondary education among upper classes in the United States, inequality at that level appears to have remained stable. Second, and related to the first point, educational expansion does not appear to have raised class inequality at the tertiary level. Arum, Gamoran, and Shavit (2007) have argued that the potential upward shift of inequality to the postsecondary level implied by MMI depends on the pace of educational expansion: in most countries, postsecondary education expanded faster than did eligibility for it,

accommodating the growing pool of applicants without tightening social selection. The same may apply to the US case.

In predicting trends in educational returns (ED) caused by educational expansion, we may distinguish two competing theoretical approaches (cf. Bills 2003; Goldthorpe 2013).² Within human capital theory (Becker 1964; Mincer 1974), theories of skill-biased technological change (SBTC) posit that, for about the past three decades, growth in labor-market sectors requiring high technical skills has outpaced the supply of highly educated workers. If educational expansion does not keep pace with the rising demand for high skills, returns to education—in particular, to higher education—increase (Goldin and Katz 2008). On the other hand, signaling (Arrow 1973; Thurow 1975) and screening theories (Spence 1973) hold that education serves to sort individuals according to their productive capacity in the labor market. Unlike human capital theory, this approach thus views education as a positional or relative good (Hirsch 1976), with its value dependent on the overall distribution of educational degrees. Educational expansion would therefore be expected to inflate the value of degrees and result in decreased returns to education. Widely cited findings of increasing income returns to education (Goldin and Katz 2008; Autor, Katz, and Kearney 2008), and particularly to college degrees, correspond to the predictions of SBTC. However, here, we are concerned with trends in *social class* returns (rather than income returns) to education—which may differ to the degree that within-class income variation has changed over time (Weeden and Grusky 2012). While empirical evidence for the United States shows a general pattern of stability in occupational returns to education when they are measured in terms of occupational status (Grusky and DiPrete 1990; Hauser et al. 2000), we know of no direct evidence on recent trends in social class returns to education in the United States. Findings from many European countries, however, show decreasing social class returns (Breen and Luijkx 2004), which are more consistent with predictions from screening and signaling theories.³

Finally, on the question of trends in the direct intergenerational transmission of status (OD), the industrialism thesis (Treiman 1970) proposes that industrial development necessarily results in a shift from ascriptive to achievement forces in the attainment of socio-economic positions. In fact, the thesis offers predictions for all legs of the mobility triad: as a functional necessity, modernization weakens the link between social origins and educational attainment (OE) while strengthening the link between educational attainment and social destination (ED), resulting in an increasing mediating role of education in processes of social mobility and, conversely, a decreasing direct effect of social origins on destinations (OD). In this perspective, educational expansion is a necessary response to the functional needs of an increasingly meritocratic industrial society. Given the ample empirical counterevidence to the industrialism thesis (Hout and DiPrete 2006), coherent alternative narratives have been surprisingly slow to emerge (see MacLean and Grusky [2014] for one attempt). Overall, we find a lack of empirical evidence that tracks changes in the direct effects of social class origins on social class destinations, independent of educational attainment, for the United States (for recent evidence for the United Kingdom, see Breen and Karlson 2014). A few contributions that

have taken a cross-national comparative approach to this topic have yielded conflicting evidence of similarity and differences in the overall role of education in processes of social class mobility across European countries (Ishida, Müller, and Ridge 1995; Breen and Luijkx 2004). In the next section, we turn our attention to the role of a more specific phenomenon in the direct transmission of social class status—namely, the way in which the direct OD association depends on the level of education attained.

The Compositional Effect

In an influential paper on trends in social class fluidity, Hout (1988) elaborated on an interaction effect that he had seen in some of his earlier mobility research (Hout 1984, 1400). A detailed analysis of intergenerational mobility based on 14 occupational categories led him to conclude that “[t]he effect of origins on destinations differs by level of education. The extreme case is college graduates. For them, current occupational status is independent of origin status. This finding provides a new answer to the old question about education’s overcoming disadvantaged origins. A college degree can do it” (Hout 1988, 1391). This finding has come to be known as the *compositional effect* in the mobility literature (Breen and Jonsson 2007, 1778). It may result from more “universal” recruitment policies in the college graduate labor market compared to occupations that do not require such credentials (Hout 1988, 1381). In effect, a college degree may be a sufficiently powerful “signal for employers that leaves little leeway for social network effects” (Breen and Jonsson 2007, 1778), which may otherwise influence recruitment into occupations characterized by low asset specificity. Moreover, the prevalence of graduate employment in bureaucratic organizations that are exemplary for rationalized procedures and limited discretion in hiring decisions could further restrict “allocative inequality,” that is, decrease the influence of ascriptive background attributes on rewards and career opportunities (Torche 2011).

Torche (2011) recently provided an overdue empirical update on the question of whether this compositional effect can also be observed for the decades since Hout’s original analyses. The short answer is yes. A college degree continues to mitigate the direct effects of socio-economic origins on socio-economic destinations in the United States—whether those are measured in social class categories or using a range of other conceptualizations, such as occupational status, individual earnings, and family income.

Based on the compositional effect, educational expansion should lead to an increase in social fluidity, since more individuals move to an educational level, bachelor’s attainment, for which social destinations are decoupled from social origins. This expectation holds in spite of an additional finding by Torche (2011) that a stronger OD association exists among those with a postgraduate degree than those with a bachelor’s degree. While the expansion of postgraduate education may tend to dampen the overall mobility-inducing effects of expansion of the postsecondary education sector, the overall effect remains positive, since both the share and expansion of postgraduate degrees are smaller than they are for bachelor’s degrees—a situation unlikely to change anytime soon.

The compositional effect has also been observed in other national contexts, including France, Germany, Great Britain, and Sweden (Vallet 2004a; Breen and Luijkx 2007; Breen and Jonsson 2007; Breen 2010). Also, some cross-national comparative work has interpreted the finding that nations with larger postsecondary sectors tend to also have higher rates of social class fluidity as indirect evidence for the compositional effect (Beller and Hout 2006b).

Hout's proposition on the importance of the compositional effect is quite bold. He notes: "I am tempted to ascribe *all the change* in inequality of occupational opportunity to the increase in college graduates in the labor force" (1988, 1384, emphasis added). In that work, he goes on to demonstrate the complexity involved in teasing out the relative contribution of the compositional effect next to changes in the other legs of the mobility triad (1384–1389). We attempt to do just that in this paper, ultimately testing whether Hout's quarter-century-old proclamation has empirical traction.

Joint Consideration

Above, we reviewed extensive research on separate aspects of the role of educational expansion in shaping social class fluidity. However, these theories and pieces of evidence are not easily pulled together into one coherent prediction and empirical test of the effects of educational expansion. Two recent contributions have made great progress along those lines.

Rauscher (2013) provides an innovative analysis using late 19th- and early 20th-century US Census data. She takes advantage of state-to-state variation in the introduction of compulsory schooling laws to identify the causal effects of educational expansion on social fluidity. Rauscher finds that the early phase of educational expansion (compulsory schooling) triggered a slight decrease in social class fluidity among those who, because of their age at the time, were required to attend only a few more years of education, but that it increased social mobility among those whose attendance was raised by the full extent defined in new compulsory schooling laws. The strengths of Rauscher's study lie in its identification of a causal effect of educational expansion on social fluidity—a quite unique addition to a literature focused on associational evidence—and in its examination of a highly interesting historical setting. However, this study—like much current research that draws causal inferences—does not empirically address the mechanisms through which these causal treatment effects occur.

Breen (2010), in contrast, has provided a new methodological approach and empirical (but non-causal) evidence on the mechanisms discussed here. Breen's decomposition method overcomes the described challenges of jointly taking into account the interdependence and possible interactions within the mobility triad and allows analysis of the relative importance of each distinct mechanism. Breen's empirical evidence, based on data from men in Sweden, the UK, and Germany, indicates a strikingly different pattern for each of these cases: social class fluidity trends have been positively influenced chiefly by educational equalization in Sweden, more by the compositional effect than by educational equalization in Germany, and solely by the compositional effect in the UK. Torche and Costa Ribeiro (2010) have applied the same methodology to Brazil, as a case of a

late-industrializing country, and found that neither of these two mechanisms account for Brazilian fluidity trends, which instead have been driven by declining class returns to education and a weakening direct effect of class origins on class destination.

Our own analyses use Breen's approach to generate the same type of evidence for the United States. We describe the benefits of the decomposition method in detail below.

Alternative Views of Social Origin

Occupation-based measures of social class continue to be influential and frequently used in sociological research. The main argument for their use is that they are not simply convenient proxy measures of other dimensions of socio-economic standing, such as permanent income (Zimmerman 1992; Hauser and Warren 1997), but that they also capture a much more extensive range of socio-economic conditions central to individuals' lives, opportunities, and consumption patterns (Wright 1996; Lareau and Conley 2008). Unlike gradational measures of inequality, such as income or earnings, the class approach seeks to account for *categorical* differences between social groups. It focuses on the relationship between positions in the system of production to provide a theoretical explanation of unequally distributed outcomes (Wright 1979). Whether the relational content of inequality is captured by mechanisms of exploitation (Wright 1997; Sørensen 2000) or by the conflictual nature of employment relations (Erikson and Goldthorpe 1992), class analysis claims to provide not only an explanatory account but also a distinct description of the inequality space: Occupations that yield similar earnings may differ widely in terms of property ownership, authority, and other aspects of employment relations (Torche 2011). In addition, when used as a descriptor of social background, the extent to which any of these factors independently impact the ability of families to facilitate their offspring's success reveals inequalities in opportunity that unidimensional measures, such as income, fail to account for.

While sharing the insight that occupation-based measures can reveal a distinct and salient dimension of inequality and opportunity, recent sociological work debates the best level of aggregation for social class measures (Weeden and Grusky 2005, 2006; Jonsson et al. 2009). Weeden and Grusky have argued that "big classes are capturing a diminishing share of the total structure in the division of labor" and that "any evidence of a weakening in [big] class effects will have to be accompanied with a caveat that such weakening may simply be an artifact of applying a measurement tool that is conveying ever less information about the inequality space" (Weeden and Grusky 2012, 1755). Their empirical evidence for this claim is debatable.⁴ While our data do not allow us to assess whether social fluidity trends are best conceptualized and measured in big, meso-, or micro-classes, it is worth noting that to date there is no empirical evidence of different social fluidity trends in the United States based on micro- and macro-class specifications (Jonsson et al. 2011, 163). Still, the suggestion that social classes have over time become a less valid way of measuring inequality and mobility taps into a common sentiment among parts of the public and among some scholars.

Beyond the theoretical defense of the class approach offered above, some of the empirical evidence we present may also challenge that view. In particular, if decreasing associations with social class measures are taken as an indication of their decreasing validity, some of our findings that document increasing associations may serve as counterevidence.

At the same time, we do not claim that social classes are the only measure of interest to capture social origins (Blau and Duncan 1967). We certainly appreciate the symmetry of an approach that draws on class measures to assess both social origins and social destinations; however, “maintaining the metaphor of social mobility” (Hauser et al. 2000, 192) should not keep us from considering other dimensions of social background that we know to have strong and independent associations with educational and occupational attainment. As Jencks and Tach (2006) remind us, “the best way to measure changes in equal opportunity is to track the effects of specific sources of intergenerational economic resemblance that offend our sense of justice” (24–25). The specific source of inequality in opportunity that we additionally take into account here is parental education.

As is the case for parental class, we may think of multiple mechanisms through which parents’ educational status shapes their offspring’s educational and occupational attainment. For instance, parents’ educational success may provide informational advantages when it comes to navigating educational careers and labor-market entry (Baker and Stevenson 1986; Lareau 1989; Pfeffer 2008). Also, parents with higher levels of education may be able to provide more resourceful learning environments at home, with their own knowledge acquired through education serving as a resource itself. Additionally, parents’ own educational status may serve as an anchor to define the minimum level of educational aspirations for their children (Breen and Goldthorpe 1997; Davies, Heinesen, and Holm 2002). Besides these theoretical reasons, we are particularly interested in the association between parents’ education and their offspring’s outcomes from an empirical perspective because (a) parental education typically has the highest independent influence among other dimensions of social background; and (b) it can be interpreted as a zero-order association between the main socio-economic background characteristics and attainment (since parental education precedes and predicts parental occupation, earnings, and income).

Empirical evidence on trends in the associations between parental education, offspring’s own educational status, and social class in the United States is rather limited and is mostly concerned with the OE leg of the mobility triad—the link between parental and children’s education, or educational fluidity. Most empirical research has indicated overall stability in the intergenerational association of education over the first three-quarters of the 20th century (Mare 1981, 1993; Hout, Raftery, and Bell 1993; Hout and Dohan 1996; Bloome and Western 2011). Trends since then have been less conclusively established. For instance, Roksa et al. (2007) observe increases in the influence of parental education on college entry during the 1980s and 1990s. Buchmann and DiPrete (2006) report gender-specific trends according to which the influence of parents’ education has increased for the higher educational attainment of their same-sex offspring. In particular, they detect a growing influence of father’s low educational status (high school attainment or less) on son’s higher educational attainment. In contrast,

Hout and Janus (2011) find overall stability in the effects of parental education, but their assessment of absolute educational mobility rates reveals decreasing rates of intergenerational upward mobility (i.e., children attaining more education than their parents) and increasing rates of educational downward mobility since the 1970s. And, finally, some findings indicate declining conditional effects of maternal education on higher educational attainment (Belley and Lochner 2007). Still, the more pessimistic conclusions about stable and potentially increasing educational inequality tied to parental education are also in line with international evidence: in most Western countries, we observe more stability in the educational attainment gaps that are tied to parental education than in those tied to parental class (Vallet 2004b; Pfeffer 2008; Shavit, Yaish, and Eval 2007; Breen et al. 2009). Evidence on cohort changes in the relationship between parental education and offspring's class attainment (ED association) in the United States is elusive. The same applies to evidence of a potential compositional effect tied to parental education. Notably, Torche's (2011) expansive analyses of the sensitivity of the compositional effect to different measures of socio-economic origins did not include parental education.

Method

First, we use log-linear and log-multiplicative models (Hout 1983; Powers and Xie 2000) to describe trends in each leg of the mobility triad. We test whether each association (OD, OE, ED) is constant across cohorts or whether we can parsimoniously describe a cohort trend drawing on the "log-multiplicative layer effects" or "uniform difference" (unidiff) model (Xie 1992; Erikson and Goldthorpe 1992). For the assessment of cohort trends in social class fluidity (OD), this model is

$$f_{ijl} = \mu \gamma_l^C \gamma_i^O \gamma_j^D \gamma_{li}^{CO} \gamma_{lj}^{CD} \exp(\Psi_{ij} \Phi_l), \quad (1)$$

where Φ_l describes the cohort-specific deviation in the association between class origin, O , and class destination, D (Ψ_{ij}). The model thus produces a single parameter (Φ_l) for each cohort that can be used to parsimoniously describe cohort trends in social fluidity (independent of the overall class distribution) while fitting a common pattern of association across all categories of O and D (Xie 1992, 382). A further, even more parsimonious model imposes linearity on the cohort trend in Φ_l , that is, a linear increase or decrease in social fluidity (Breen and Luijkx 2004).⁵

We draw on a decomposition method developed by Breen (2010) to compare observed trends in social fluidity to counterfactual trends derived from specific assumptions about the role of education (for a detailed description of the method, see appendix A). In particular, we assess how educational expansion has altered social mobility trends through the following three mechanisms: the compositional effect (OED), cohort changes in class inequality in education (COE), and cohort changes in the returns to education (CED). We begin with a baseline counterfactual mobility trend that constrains all meaningful interactions between

origin, education, and destination to be constant across cohorts—resulting in a flat trend. We then investigate mobility trends based on counterfactual COD distributions that we derive from separately freeing up the parameters entailing the three mechanisms described above. We then assess how closely each of these counterfactual mobility trends approximates the observed mobility trend. This decomposition approach thus allows us to gain an insight into the relative importance of each mechanism through which educational expansion may contribute to social mobility trends.

All of our models are estimated using the program R (R Core Team 2013; Turner and Firth 2012).

Data and Measures

We draw on data from 29 repeated cross-sectional surveys from the General Social Survey (GSS) administered between 1972 and 2012. Our analytic samples ($N = 14,588$ – $14,608$) consist of men age 30 to 64 in each year of data collection. We follow Breen (2010) in restricting our analyses to men and compare our results to his male-only evidence from other countries. The exclusion of women is lamentable but imposed by data limitations. Female labor-force participation was low in the earliest cohorts, yielding too few observations to reliably model their fluidity trend and, even less so, to account for the mechanisms underlying it. In appendix B, we discuss these data limitations in more detail but also provide suggestive baseline estimates for women that await more reliable modeling in future research.

We further restrict our analytic sample to respondents who had lived in the United States at age 16 to capture those most likely to be exposed to educational expansion in the United States (as opposed to those who have attained their basic education outside the United States).

Our analyses distinguish six birth cohorts, covering men born roughly before and during WWI (1883–1921), in the interbellum period (1922–1933), before and during WWII (1934–1945), post-WWII (1946–1957), during the Fordist growth phase of the 1950s and 1960s (1958–1969), and during the recession era of the 1970s and early 1980s (1970–1982).⁶ We typically label our cohort members by the years they turned 30 to help focus on the time period in which they completed their educational and occupational attainment.

Men's educational attainment is measured as the highest degree attained in the following four categories: less than high school, high school, some college (including associate's degree), and bachelor's degree or higher. As a measure of social origin, we use the highest educational status of parents based on the same degree categories. Because our sample sizes do not allow us to distinguish between the attainment of a bachelor's degree and postgraduate degrees for our decomposition analysis, the results can be understood as a weighted average of the educational inequalities, returns to education, and most of all, the compositional effect (Torche 2011) associated with each of the two levels of postsecondary attainment.

Respondents' reports of their current occupation are the basis for the assessment of their social class destination. Drawing on the EGP class scheme (Erikson

and Goldthorpe 1992), which has been widely used in quantitative research on social class, we distinguish six occupation-based social classes: higher-grade professionals and managers/higher service class (I), lower-grade professionals and managers/lower service class (II), routine non-manual workers (IIIab), the self-employed and farmers (IVabc), skilled manual workers and supervisors (VI + V), and unskilled manual workers (VIIab).⁷

We assess men's class origin by applying the same EGP class scheme to respondents' reports of their father's occupation when they were growing up. We use father's occupation as the measure of origin because information on mother's occupation is not available in earlier years. While this focus on male lineages again follows most prior research, we acknowledge that estimates of class mobility trends in the United States for the most recent cohorts vary depending on how mother's social class status is taken into account (Beller 2009). In stability analyses reported in Online supplement, we replicate some of those findings, but the study of the mechanisms behind these diverging trends through our decomposition approach is beyond the scope of this paper and the statistical power of our sample. Based on Beller's findings, we note that we may underestimate a recent decrease in social class mobility.

We impute missing values on our main measures of education, destination, and origin using the Stata *mi* command. The results reported here are stable to a wide range of different approaches to treating missing values, different specifications of our social class measure, and different sample constructions. These stability analyses are reported and discussed in more detail in Online supplement.

Changes in the Educational and Class Structure

We begin by describing cohort trends in the two societal features that are at the heart of this assessment: the educational structure and the class structure (see table 1).

In terms of shifts in the educational distribution, ample empirical research has of course already described the rapid pace of educational expansion during much of the 20th century and its tapering off during the past decades (Fischer and Hout 2008; Goldin and Katz 2008; Garfinkel, Rainwater, and Smeeding 2010). Our own data capture these trends well: the share of men with a postsecondary degree rose rapidly and linearly from 12.9 percent in the first cohort (who turned 30 before 1951) to 30.9 percent in the fourth cohort (who turned 30 between 1976 and 1987). Since then, however, the share of postsecondary degree holders has remained stable, as has the share of those with some college. These trends are mirrored at the lower level of the educational distribution, where high school dropout rates decreased sharply and linearly for the first four cohorts (from 44.9 to 11 percent) and then remained at that level for the last two cohorts. These trends, once again, underline the dramatic success in expanding education during most of the last century and the ebbing of that trend in recent decades. In the remainder of the manuscript, we therefore use the term "educational expansion" as a shorthand for these large-scale distributional shifts in education without meaning to imply a linear progress for all cohorts (see also Goldin and Katz 2008, 249).

The second panel of table 1 shows cohort changes in the class structure during the same period. Highly skilled white-collar positions (high service class)

Table 1. Cohort Trends in Education and Class Structure

	Year turned 30					
	Before	1952–	1964–	1976–	1988–	2000–
	1951	1963	1975	1987	1999	2012
Highest degree						
Less than HS	44.9%	35.8%	20.7%	11.0%	9.8%	10.5%
HS	41.8%	43.7%	50.1%	50.5%	53.6%	50.2%
Some college	0.4%	1.8%	4.2%	7.6%	8.5%	7.6%
BA and more	12.9%	18.7%	25.1%	30.9%	28.1%	31.6%
	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%
Destination class						
High service	16.4%	22.1%	25.6%	23.7%	23.3%	22.0%
Low service	11.2%	11.4%	13.4%	16.4%	15.9%	18.5%
Routine non-manual	8.7%	9.7%	7.3%	8.2%	7.8%	9.2%
Self-employed	14.3%	12.3%	10.7%	9.3%	8.1%	7.2%
Skilled manual	24.8%	20.5%	20.2%	20.2%	22.5%	21.8%
Unskilled manual	24.6%	24.0%	22.7%	22.2%	22.4%	21.4%
	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%

expanded substantially for the first three cohorts of our sample (from 16.4 to 25.6 percent) but then slowly declined to 22 percent for the most recent cohort. On the other hand, the share of lower-grade professionals and managers (low service class) rose steadily from 11.2 percent in the oldest cohort to 18.5 percent in the youngest cohort. The working classes experienced a contraction that was comparatively modest in size and mainly due to the decreasing share of unskilled manual labor (from 24.6 to 21.4 percent). Routine non-manual labor shows no pronounced cohort trends. The share of self-employed (including farmers) has been cut in half (14.3 to 7.2 percent), mostly due to the rapid decline of small-farm holders. Given these changes in the class structure, it is important to emphasize that our analyses assess relative mobility rates (“exchange mobility”). These rates capture social fluidity levels that exclude mobility that directly follows from the structural shifts shown here (“absolute mobility”).

Educational Expansion and Social Class Fluidity

Observed Trends

We begin by assessing cohort trends in each leg of the mobility triad. Table 2 reports the fit statistics for three different models of trends in men’s social class fluidity (ODC), trends in educational inequality tied to parental class (OEC), and

Table 2. Parental Class: Fit Statistics for Observed Trends in Mobility Components

	G^2	df	p	Δ	BIC	p	
						vs #.1	vs #.2
ODC (trends in social class fluidity)							
1.1 Constant	150.1	125	0.0623	0.037	-1,049		
1.2 Linear UniDiFF	146.7	124	0.0804	0.036	-1,042	0.0652	
1.3 UniDiFF	146.3	120	0.0516	0.036	-1,004	0.5786	0.9825
OEC (trends in educational inequality tied to parental class)							
2.1 Constant	101.8	75	0.0214	0.027	-617		
2.2 Linear UniDiFF	100.8	74	0.0208	0.027	-609	0.3173	
2.3 UniDiFF	98.1	70	0.0150	0.027	-573	0.5934	0.6092
EDC (trends in class returns to education)							
3.1 Constant	105.4	75	0.0119	0.024	-614		
3.2 Linear UniDiFF	105.2	74	0.0101	0.024	-604	0.6547	
3.3 UniDiFF	94.1	70	0.0291	0.023	-577	0.0458	0.0255
UniDiff parameters	Linear	C = 1	C = 2	C = 3	C = 4	C = 5	C = 6
OD (1.2 & 1.3)	-0.030	1	0.946	0.913	0.889	0.843	0.864
OE (2.2 & 2.3)	0.021	1	0.884	1.024	0.979	1.030	1.030
ED (3.2 & 3.3)	-0.006	1	1.032	1.161	1.035	1.101	0.945

Note: Authors' calculations based on GSS (1972–2012); $N = 14,608$.

trends in class returns to education (EDC). In each case, the first model assumes the association to be constant across cohorts, the second model imposes a linear cohort trend in the strength of association (while holding the pattern of association constant), and the third model allows the strength of association to differ freely across cohorts.

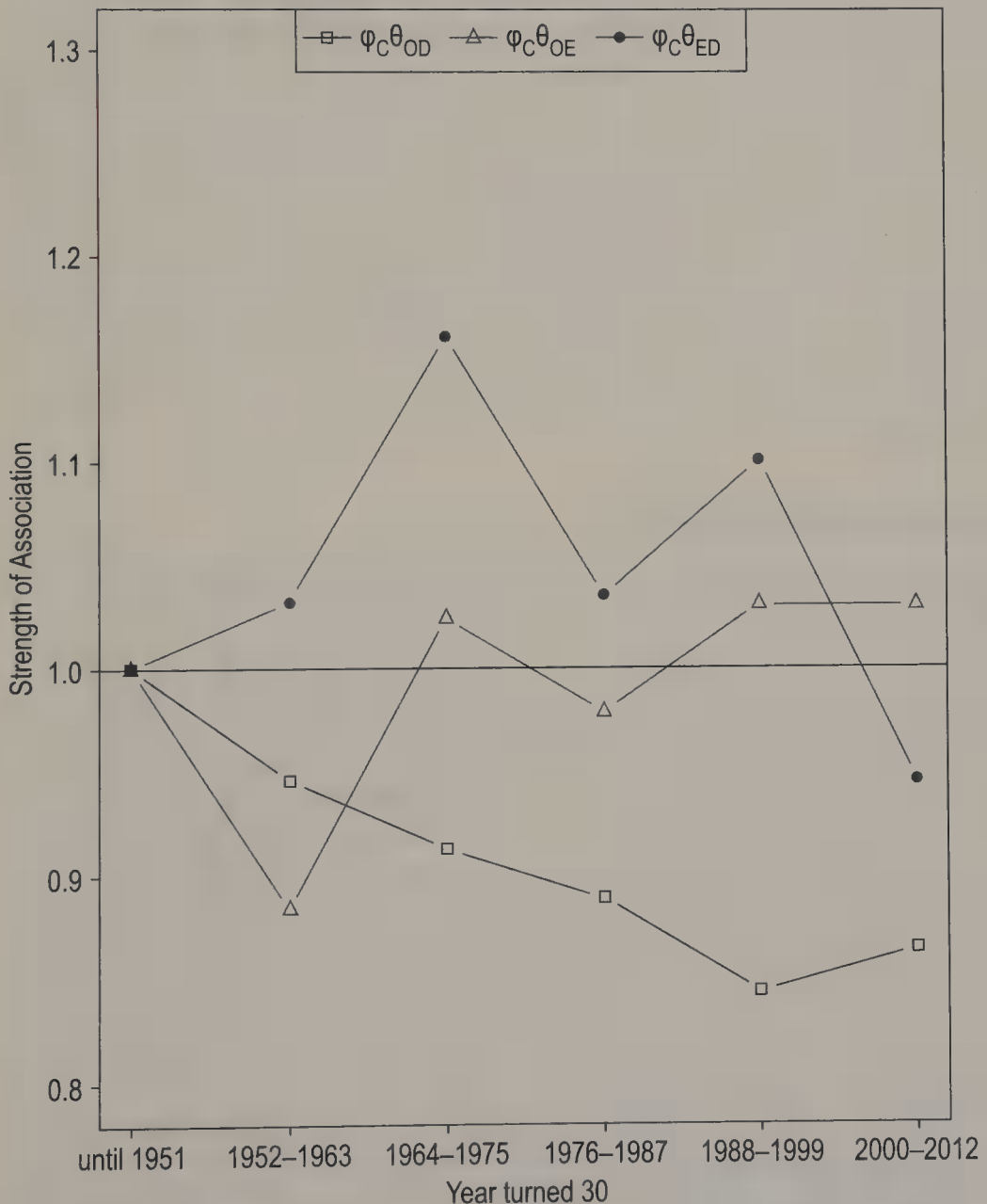
For all three processes studied, the first model (no trend) generally yields a satisfactory model fit with non-significant deviations between predicted and observed frequencies at $p > 0.01$ – 0.06 and the share of misclassified cases (Δ) between 2.3 and 3.7 percent. It could be chosen as the preferred model when also considering its parsimony (BIC values 7–10 points lower compared to linear unidiff models). In general, this finding indicates that cohort trends in these associations are of relatively modest size, if present at all.

In fact, for the description of cohort trends in class inequality in education, we can accept the model of constant association as the best model. In contrast, for the analyses of both social fluidity and returns to education, we find some evidence for modest cohort differences. For trends in social class fluidity, the linear unidiff model provides an improvement in model fit over the constant association model ($p < .065$). The bottom panel of table 2 shows a negative linear unidiff parameter for social fluidity (-0.030), denoting a linearly decreasing association between class origin and class destination across cohorts. For trends in class returns to education, the unidiff model also provides a significant improvement in model fit over the constant association model ($p < .046$),

although the cohort differences do not follow a clear pattern (bottom panel of table 2).

In plain terms, we find stability in class inequality in education, modest but continuous increases in social class fluidity, and relatively trendless fluctuation in class returns to education. Although not the preferred model in all cases, the unconstrained unidiff models yield parameter estimates that reinforce this conclusion. We display these unidiff parameters in figure 2 for further illustration.

Figure 2. Parental class: Observed trends in mobility components



Note: Displaying UniDiff parameters for trends in social class mobility ($\varphi_C\theta_{OD}$), class inequality in education ($\varphi_C\theta_{OE}$), and class returns to education ($\varphi_C\theta_{ED}$) in reference to the oldest cohort. Fit statistics and number of observations see table 2.

First, we can observe a linear decline in the association between class origin and destination (θ_{OD}) up to the youngest cohort. The uptick in the OD association for the youngest cohort can be interpreted as a tentative sign of a recent decline in fluidity (it is also more pronounced, though still not statistically significant, when we impose a lower age limit of 35 instead of 30; available from the authors). In particular, we point out that this suggestive evidence coincides with the findings by Mitnik, Cumberworth, and Grusky (2013), despite being based on a much different analytic approach.⁸ Second, we observe overall stability in the association between class origin and education (θ_{OE}). The seemingly lower level of class inequality in educational attainment among the second oldest cohort could reflect the egalitarian effects of the GI bill (Bound and Turner 2002)—although this evidence is again at best suggestive at the backdrop of the overall fit statistics discussed above (no significant improvement of the unidiff model over the “no trend” model). And, third, we see largely directionless fluctuation across cohorts in the association between education and class destination (θ_{ED}).⁹

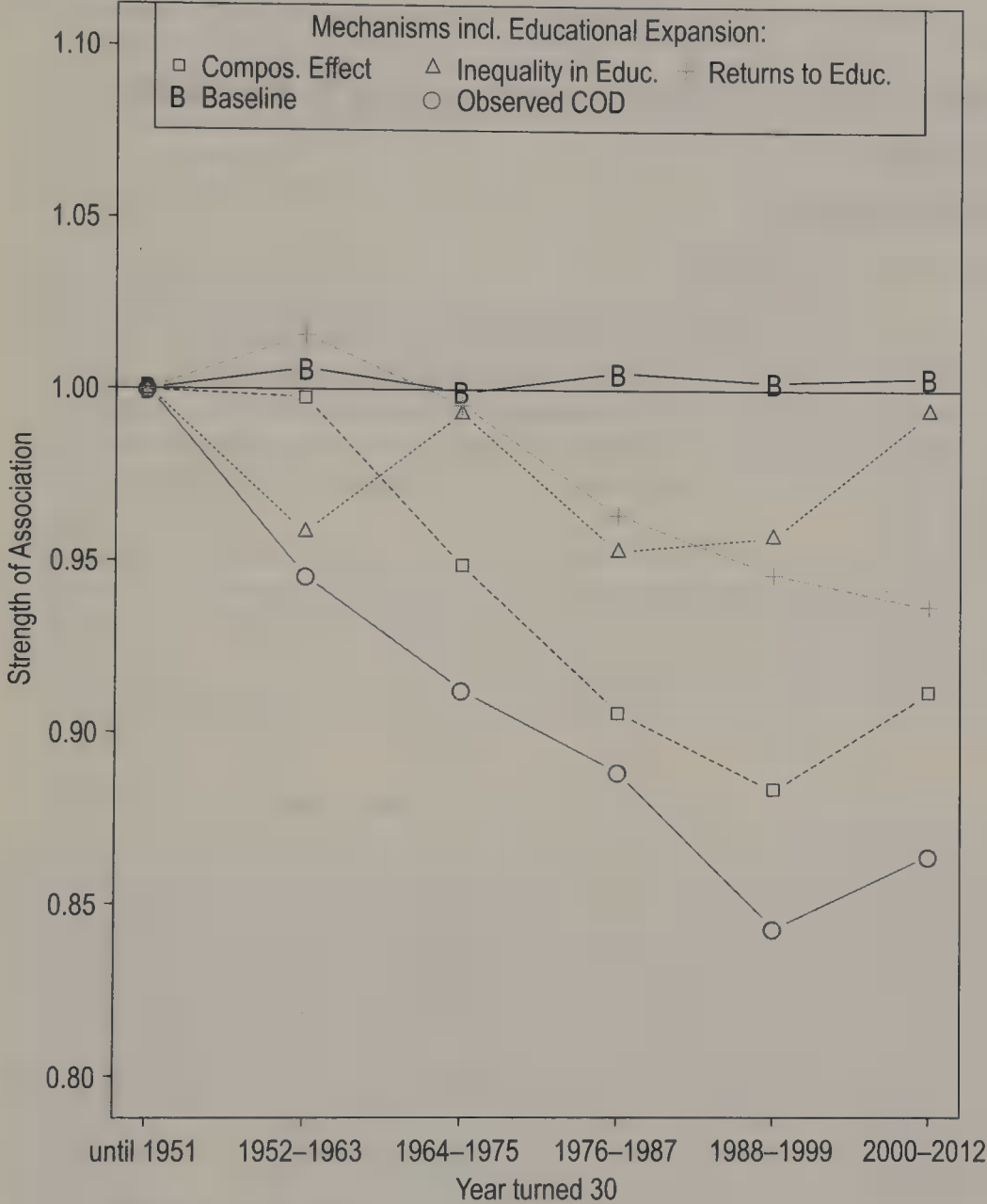
The evidence presented here is new and interesting in its own right, indicating that while men’s social class fluidity increased modestly across birth cohorts over the past seven decades, class inequality in education and class returns to education did not vary consistently across cohorts. However, as explained above, these findings yield informative but insufficient evidence regarding the overall role of education and its expansion in explaining the increase in social class fluidity. To investigate how these three trends fit together, we therefore now turn to the decomposition analysis.

Decomposition Analysis

The aim of our decomposition analysis is to reveal the relative importance of each of the three mechanisms through which educational expansion may shape trends in social class fluidity. In essence, we are interested in how closely each counterfactual fluidity trend approximates the observed trend. Figure 3 displays the observed trend in social fluidity (O) that we already described above. The baseline fluidity trend (B), based on a counterfactual mobility table derived from a model that fixes all relevant parameters to be constant (see Appendix A), unsurprisingly is one of stability. Our main interest lies in the three lines between the baseline (B) and the observed mobility (O) trend. Each of these trend lines is based on a counterfactual mobility table that fits the effects of educational expansion in combination with one of the following: the compositional effect (\square), changes in class inequality in education (\triangle), and changes in returns to education ($+$), respectively. We assess the relative importance of these three mechanisms by observing how closely each corresponding counterfactual trend approximates the observed trend.

The result is quite clear: the counterfactual trend that best and quite closely approximates the observed trend is that produced by the model that accounts for the effect of educational expansion via the compositional effect (\square). The flattening out of this counterfactual trend for the last three cohorts also corresponds well to the demonstrated slowdown in educational attainment among those cohorts. In contrast, class inequality in education and class returns to education,

Figure 3. Parental class: Counterfactual trends in social mobility



Note: Displaying UniDiff parameters from separate models fitted to counterfactual and observed cross-classifications of parental class, education, and class destination (see Appendix A).

which we found to be largely trendless, do not contribute to increases in social fluidity. Our graphical inspection thus leads us to conclude that the compositional effect alone accounts for the positive relationship between educational expansion and social fluidity rates - a conclusion very much in line with Hout's original hypothesis (1988). We can further quantify this effect using our decomposition results: based on the linear unidiff parameters, 90 percent of the observed trend in social class fluidity is tied to the effects of educational expansion via the compositional

effect (linear unidiff parameter estimates for baseline = 0.000, compositional effect = -0.027, observed = -0.030).

A Complementary View: Inequalities tied to Parental Education

Observed Trends

We now turn our attention to a view of social fluidity that relies on a different indicator of socio-economic origin - parents' educational attainment. As discussed above, there are sound theoretical reasons to expand our assessment of trends in educational and class attainment opportunities in this direction. Also, from an empirical perspective, we are interested in describing an alternative dimension of attainment inequality that may have been subject to a different development over time than that based on father's occupational status.

Our empirical models, which mirror those presented above, now capture inequality in educational attainment and class attainment (measured in the same way as above) tied to parental education (OD and OE, respectively) and, as before, class returns to education (ED). Table 3 reports the fit statistics of our main models for each of these associations. We will not repeat our discussion of

Table 3. Parental Education: Fit Statistics for Observed Trends in Mobility Components

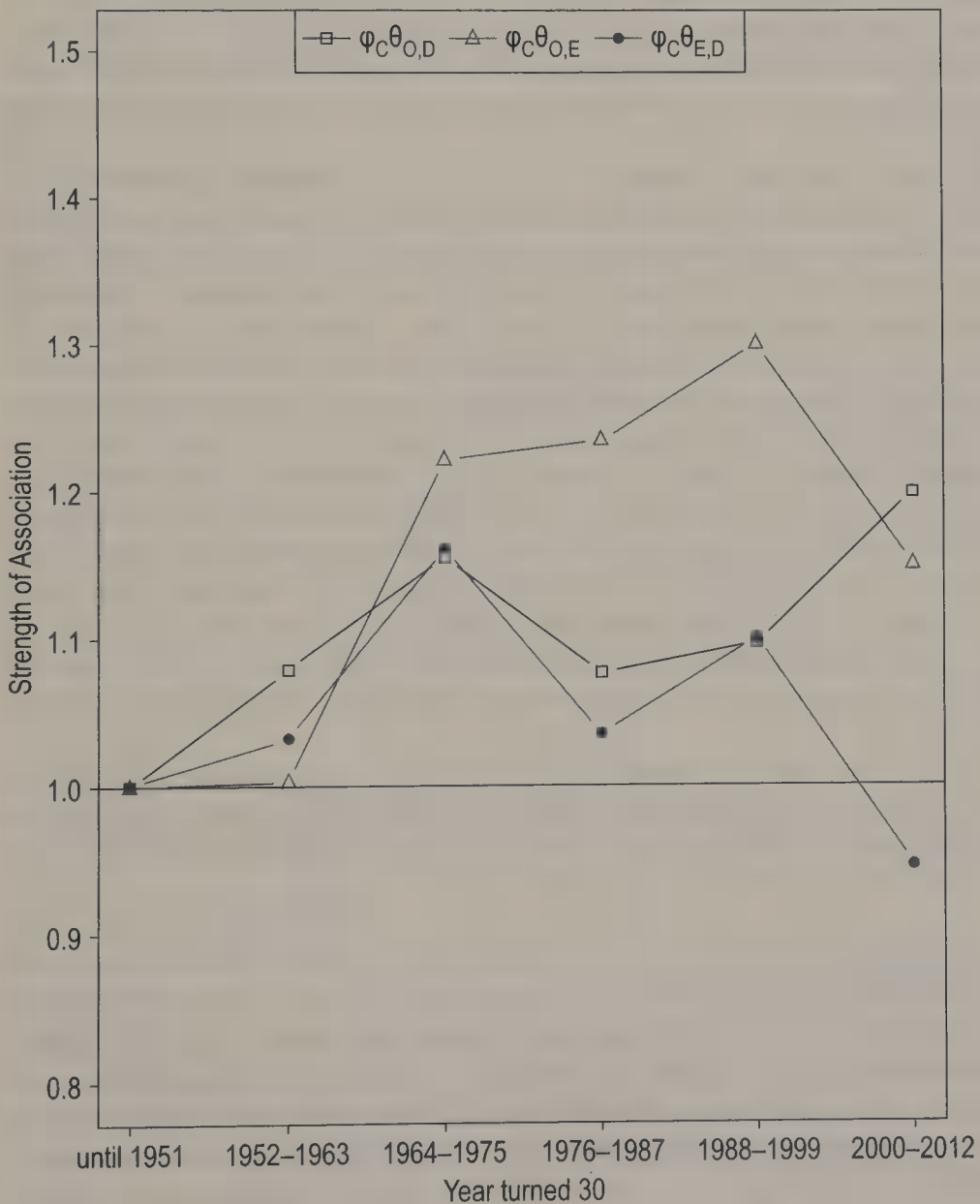
							<i>p</i>	
							vs #.1	vs #.2
		<i>G</i> ²	df	p	Δ	BIC		
ODC (trends in association between parental education and class destination)								
1.1	Constant	56.1	75	0.9496	0.018	−663		
1.2	Linear UniDiFF	55.8	74	0.9434	0.018	−654	0.5839	
1.3	UniDiFF	54.1	70	0.9204	0.018	−617	0.8492	0.7907
OEC (trends in educational inequality tied to parental education)								
2.1	Constant	70.5	45	0.0090	0.017	−361		
2.2	Linear UniDiFF	61.5	44	0.0414	0.014	−360	0.0027	
2.3	UniDiFF	53.1	40	0.0805	0.012	−330	0.0038	0.0780
EDC (trends in class returns to education)								
3.1	Constant	105.4	75	0.0119	0.024	−614		
3.2	Linear UniDiFF	105.2	74	0.0101	0.024	−604	0.6547	
3.3	UniDiFF	94.1	70	0.0291	0.023	−577	0.0458	0.0255
UniDiff parameters		Linear	C = 1	C = 2	C = 3	C = 4	C = 5	C = 6
OD (1.2 & 1.3)		0.014	1	1.079	1.155	1.077	1.097	1.199
OE (2.2 & 2.3)		0.060	1	1.003	1.222	1.234	1.300	1.149
ED (3.2 & 3.3)		−0.006	1	1.032	1.161	1.035	1.101	0.945

Note: Authors' calculations based on GSS (1972-2012); N = 14,588.

cohort trends in class returns to education, since the models estimated for this association are equivalent to those reported earlier.

These new analyses reveal trends that sharply differ from those described earlier. For our assessment of the association between parental education and class destination (OD), the constant association model provides a great fit upon which neither the linear unidiff nor the unidiff model may improve, suggesting that cohort trends in this association should be minimal. The unidiff estimates (bottom of table 3 and displayed in figure 4) yield a slightly different impression, namely, one of a generally increasing association. A conservative interpretation

Figure 4. Parental education: Observed trends in mobility components



Note: Displaying UniDiff parameters for trends in association between parental education and class destination ($\varphi_C\theta_{OD}$), inequality in education tied to parental education ($\varphi_C\theta_{OE}$), and class returns to education ($\varphi_C\theta_{ED}$) in reference to the oldest cohort. Fit statistics and number of observations see table 3.

of this evidence suggests that, unlike the influence of class origin, the influence of parental education on class destination has not declined, and may even have increased somewhat.

The statistical evidence on trends in inequality in educational attainment tied to parental education (i.e., educational mobility) is more clear-cut. Both the linear unidiff model and the unidiff model yield statistically significant improvements in model fit over the constant association model ($p < .003$ and $p < .004$, respectively), and figure 4 reveals a pronounced upward trend in this association with a decline for the youngest cohort (a very similarly shaped, though non-significant trend, is reported in Pfeffer [2008], 552).

In sum, while we earlier showed a relatively weak but steady decrease in the association between men's class and their social origin based on father's class, here we find no such decrease based on parental education. In terms of men's own educational attainment, we even see evidence of an increasing role of parents' educational status.

These conclusions about trends in the distribution of opportunities thus differ quite substantially from those derived earlier based on a measure of social class origin. Ascribing these differences to the decreasing validity of "big" social class measures would overlook the fact that, in both sets of analyses, our measure of socio-economic destination is social class. That is, if decreasing associations between class backgrounds and class destinations are interpreted as indicative of decreasing validity of social class measures, stable or increasing associations between educational background and class destinations would suggest stable or increasing validity of our measure of social class destinations. Instead, we believe that the results just presented imply that the structuration of life chances in terms of class destinations continues and is increasingly tied to educational backgrounds.¹⁰ Rising intergenerational associations in educational attainment may, for instance, reflect increased information requirements to navigate a greatly expanded set of educational options, which may be best met by parents' own knowledge and experience of the educational system (see also Baker 2014, 54).

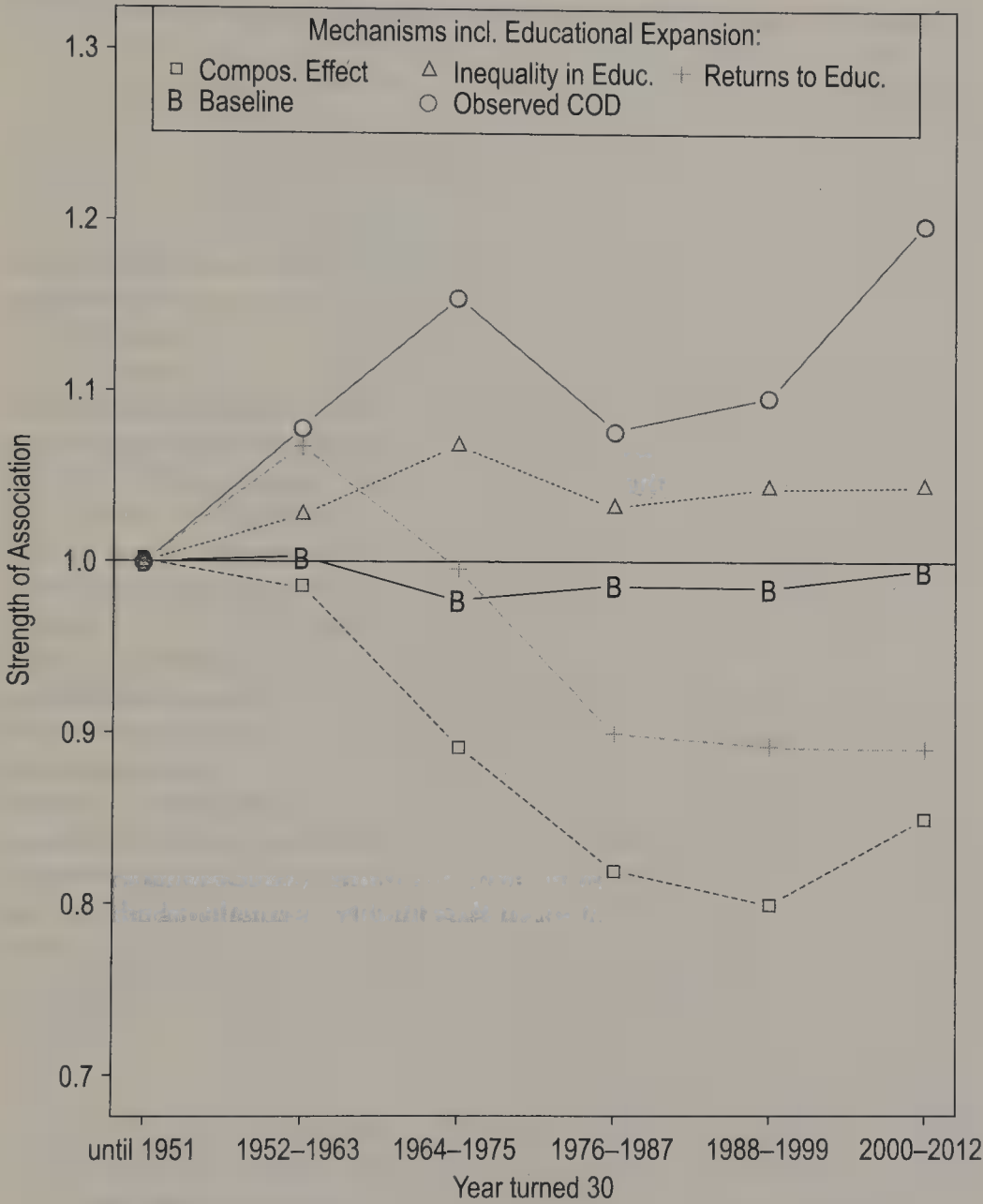
Overall, the evidence presented here thus provides an important and cautionary tale about mistaking increasing levels of social class mobility, analyzed as intergenerational associations in social class, for a general and broad trend toward more fluidity.

Decomposition Analysis

The divergent trends in social fluidity based on parental education versus father's class immediately raise the question of which way the three mechanisms assessed earlier can explain the observed trends in this dimension of inequality in opportunity. Results from the decomposition analysis again provide a clear-cut answer (see figure 5).

Educational expansion *decreased* the association between parental education and sons' class destination through the compositional effect (\square), and, at least for the youngest three cohorts, through changing class returns to education (+); although, as shown above, the latter changes show no consistent pattern. In

Figure 5. Parental education: Counterfactual trends in social mobility



Data: GSS 1972-2012.

Note: Displaying UniDiff parameters from separate models fitted to counterfactual and observed cross-classifications of parental education, education, and class destination (see Appendix A).

other words, these two mechanisms worked in the opposite direction of the actually observed trends. The strong positive influence of the compositional effect—similar to its major role in class fluidity trends—has been counterbalanced by a simultaneous increase in educational inequality (documented above); the counterfactual changes in fluidity generated by the latter trend (△) closely track the observed changes. That is, educational expansion has increased educational

inequalities tied to parental education to an extent that offsets the otherwise strong positive influence of the compositional effect.¹¹

Conclusion

Many agree upon increasing intergenerational social mobility as a policy goal, whether out of consideration of social justice or economic efficiency. The education system has long been acknowledged as a central sphere for the provision of such opportunities for mobility (Durkheim 1965[1922]; Coleman 1968; Labaree 1997). In this analysis, we expand on recent work (Breen 2010) to shed new light on the role of education for social mobility by tracking the relationship between shifts in the educational distribution and long-term trends in social fluidity for the United States. We identify three main channels through which educational expansion may shape social fluidity trends - changes in educational inequality, changes in returns to education, and the compositional effect - and we test their relative importance in accounting for cohort changes in social fluidity.

In terms of social class fluidity, we find a slow but steady increase across cohorts born throughout the first eight decades of the 20th century. This finding constitutes a separate contribution to an ongoing controversy about long-term trends in social class fluidity (Long and Ferrie 2013; Xie and Killewald 2013; Hout and Guest 2013). We find that the fluidity-inducing effects of educational expansion are nearly entirely accounted for by the compositional effect (the fact that the direct link between social origins and destinations is severed among those men who attain a college degree; see Hout 1988; Torche 2011). Because educational expansion has increased the share of the population with college degrees, its effect on social fluidity has been positive. In contrast, educational expansion did not contribute to higher rates of social class fluidity by equalizing educational outcomes. We have shown, in line with prior evidence, that it left the degree of class inequality in education largely unchanged. Also, increasing levels of social class fluidity cannot be ascribed to the way in which educational expansion has impacted class returns to educational degrees, whose development across cohorts does not follow a consistent pattern.

Hout and Dohan have argued that “the lack of coordinated or sustained policy regarding equality of educational opportunity makes the United States a prototypical example of a nation that has relied on expansion more than policy to promote equality of educational opportunity” (Hout and Dohan 1996, 212). Like others before us, we have shown that educational expansion alone is a poor policy when aimed at decreasing class inequality in education, which has persisted quite stubbornly. While more targeted policies are needed to decrease inequality in education in the United States (Haveman and Smeeding 2006), our findings suggest that educational expansion has nevertheless contributed in a major way to increasing men’s mobility opportunities (see also Breen 2010, 382). As hypothesized a quarter century ago by Hout (1988), the greatest part of the increase in social fluidity (in our empirical results, 90 percent) can be ascribed to the compositional effect.

In comparison to the three national cases presented by Breen (2010), our findings for the United States are most akin to the results from the United Kingdom,

where the compositional effect emerged as the only fluidity-inducing factor. The evidence that the compositional effect is the near exclusive channel through which educational expansion has positively influenced social class fluidity in these two Anglo-Saxon countries corresponds to findings from cross-national comparative work that has hypothesized that the compositional effect is “most pronounced in the liberal welfare setting where the association would otherwise be greatest” (Beller and Hout 2006b, 353).

We have argued that despite a long sociological tradition of assessing trends in mobility opportunities tied to parents’ social class, there are sound reasons to be interested in other dimensions of socio-economic inequality in opportunity as well. Having analyzed inequalities tied to parental education, we are wary of broad claims about social progress solely based on analyses that use just one select measure of social origin. Unlike the picture of moderately growing social fluidity generated by our analyses based on parents’ occupational status, considering the role of parental education for sons’ educational and class attainment offers a less dynamic view. We find no evidence for a decrease in the association between parental education and sons’ class attainment (social fluidity); if anything, this association has increased over time. Also, the intergenerational association in educational status has increased, effectively counterbalancing the positive compositional effect tied to parental education.

The divergent trends in educational inequality and social fluidity based on two different indicators of social origins may indicate that the channels of intergenerational status transmission change across time (Sorokin 1927). While one dimension of socio-economic advantage may lose some power in enabling the success of the next generation, another dimension of advantage may gain traction. Based on the findings presented here, such a story could be told about social class inequalities in opportunity making way for inequalities tied to parental education (Bourdieu 1984, 1996). However, our conclusions about the role of educational expansion hold for both of these divergent trends: Educational expansion contributed to more equal opportunities for social class attainment by breaking the direct intergenerational transmission of status among an increasing number of men attaining a college degree—and by thereby counterbalancing the stability of class inequalities in educational attainment and the increase of educational inequalities tied to parental class.

The findings presented here can inform future research seeking to address related topics and unresolved questions. First, our finding of divergent trends tied to parental class and parental education raises the question of how other dimensions of inequality in opportunity have evolved over time. Another such dimension is income, the subject of a large field of research with some partial evidence that awaits integration. For example, recent research indicates that the intergenerational income correlation has remained largely stable (Lee and Solon 2009; Chetty et al. 2014); that income inequality in education has recently been increasing (Belley and Lochner 2007; Reardon 2011; Bailey and Dynarski 2011); that income returns to education have experienced a great upswing; and that the compositional effect also holds for this dimension (Torche 2011). Noting parallel trends in some legs of the mobility triad (Mazumder 2012; Corak 2013) is still a far cry from establishing the overall role of education and educational expansion

in explaining income mobility trends. Significant progress in this direction has been made by Bloome and Western (2011), but the relative role of the compositional effect in explaining changes in income mobility has not yet been studied.

Second, switching from occupation-based measures to income measures entails more than merely empirical curiosity—it requires us to cross a well-maintained chasm between categorical and gradational approaches to inequality and mobility (Weeden and Grusky 2012). One specific version of the categorical view that is still tied to occupations (and therefore comes with an explanatory rather than merely a descriptive aim) has been provided in the “micro-class” approach (Weeden and Grusky 2005). We have no issue with the claim that associations of attitudes, lifestyles and sentiments is more closely aligned with “micro-classes” than “big classes” (see also Chan and Goldthorpe 2007). We are, however, less convinced that existing empirical evidence suggests a decreasing structuration of life chances in the economic terrain along broadly defined class lines. In particular, we do not view decreasing intergenerational associations in (big) social class measures as an indication of decreasing validity of class measures. In fact, our finding of an increasing association between parental education and son’s class attainment would allow claiming the contrary.

Third, our mobility analyses are limited to male-only lineages. Future work will have to establish whether the processes studied apply in a similar fashion to women’s fluidity patterns as well as to the effects of mother’s social class (but see also Appendix B and Online Supplement). Such work is highly relevant because educational expansion has followed different patterns for males and females (DiPrete and Buchmann 2006; Hout and Janus 2011) and because the speed and extent of the increase in educational participation among women make for a particularly interesting case to assess the underlying mechanisms. In particular, a future analysis of women may further strengthen or undermine our finding of the dominant role of the compositional effect in driving trends in class fluidity.

Fourth, we stress that—like most other mobility research—our evidence is associational. We have followed Breen (2010) in focusing on a mechanistic explanation of the impacts of educational expansion to the detriment of causal inference. Methodological tools to unite causal inference methods and mechanistic approaches are developing (e.g., der Weele and Robins 2007; Knight and Winship 2013), so we may in the future get closer to identifying causal mechanisms. But in the context of the questions studied here, we will likely not be able to draw on long-term historical trends but will instead need to investigate the effects of specific institutional and policy changes that significantly and abruptly altered education systems (Rauscher 2013).

Notes

1. For a detailed study of the single legs of the mobility triad in eight European countries, see Pollak (2009).
2. Of course, there are a range of additional theories about the factors accounting for trends in educational returns that are not directly connected to changes in the educational structure but that locate these factors primarily in labor-market changes, such as those caused by rent-seeking behavior (e.g., MacLean and Grusky 2014).

3. Again, alternative explanations focusing on labor-market changes are feasible. For instance, Jackson, Goldthorpe, and Mills (2005) argue that structural changes toward service-oriented occupations have lowered the importance of technical skills but increased the demand for personal skills for which education credentials become “a ‘more’ noisy signal than previously” (Jackson, Goldthorpe, and Mills 2005, 12–13). The expansion of the service sector should consequently produce a decreasing association between educational attainment and social class attainment (see also Goldthorpe 2013, 15–16).
4. Unlike Weeden and Grusky’s analysis of the role of micro-classes in capturing lifestyles, sentiments, and dispositions, their claim about the superiority of micro-class measures with regard to life chances and material inequality—here measured as the combination of educational credentials, parental education, income, homeownership, employment level, and subjective financial position—overstates their own empirical evidence (Weeden and Grusky 2012). Their results (1742) suggest that material inequalities are more highly associated with macro-classes than with micro-classes (60 versus 40 percent of total association, respectively). Furthermore, their trend analysis (1747) reveals little difference across periods between the association of life chance indicators and either micro-classes (0 percent change) or aggregated classes (2 percent increase). These results have not gone unnoticed by the authors—for instance, when they state that “[t]he life chances domain is the only one in which big-class gradationalism is increasing, whereas the general trend is of declining big-class gradationalism” (1754). For another critical perspective, see also Erikson, Goldthorpe, and Hällsten (2012).
5. We have also fitted models that additionally allow the pattern of the association to differ across cohorts (Goodman and Hout 1998). The improved model fit, however, comes at too high a cost in terms of parsimony, as suggested by the BIC statistic (not shown).
6. The available data force us to use cohorts that are not age standardized, that is, the younger cohorts are observed, on average, at younger ages. This could raise the concern that we may underestimate the class status in younger cohorts since more of these cohort members may not have reached their final or typical class position. We test for this potential bias, within the strict limits of data availability, by increasing the lower age limit from 30 to 35 (anything beyond that would leave us with too few observations). These stability analyses (available from the authors) do not yield substantively different conclusions about the observed cohort trends. Furthermore, since the cohorts are drawn from different GSS waves, we have performed additional analyses to assess the stability of our findings against potential survey effects. The results show that the fluidity trends are not substantially altered if survey year is considered (available upon request).
7. The GSS occupational coding scheme changed from 1970 to 1980 Census Occupational Classifications during the late 1980s. This change has hindered the assessment of long-term social mobility trends spanning all GSS waves in prior research (e.g., Beller and Hout 2006a). We draw on crosswalks from 1970-based to 1980-based EGP codes that have been developed and fruitfully applied in prior research (Hertel and Groh-Samberg 2014; for a similar effort, see Mitnik, Cumberworth, and Grusky 2013) and validate the resulting measures through a comparison of social mobility trends based on three double-coded GSS waves (results available from the authors). The GSS is currently engaged in a large-scale project to recode occupational information in all waves using a common occupational coding scheme, which will allow future research to circumvent these crosswalk procedures.
8. We have opted for a cohort analysis of overall fluidity trends because our main analytic interest lies in the relationship between broad trends in social fluidity and shifts

in education, which should exert a common influence on those attending school around the same time. In contrast, Mitnik, Cumberworth, and Grusky apply a mixed period-age design with a focus on selected cells of the mobility tables because they are interested in tracking the impact of the recent takeoff in economic advantage at the top of the class distribution. The clearest indication of decreasing social fluidity that they present applies to the increasing reproduction of professional/managerial status (EGP I and II) among the youngest individuals.

- 9. We have noted earlier that this finding is in line with international evidence and also not at odds with the widely acknowledged finding of growing income or earnings returns to education. The coexistence of these two trends (which we are also able to replicate based on our own data; available from the authors) is explained by the increasing earnings gap between low-paying and high-paying occupations that underlies much of the overall increase in income inequality (Mouw and Kalleberg 2010; Wright and Dwyer 2003). Take higher managers as an example. Educational credentials have always been and continue to be important to enter these positions. At the same time, the earnings of higher managers have increased disproportionately relative to the rest of the earnings distribution (Mouw and Kalleberg 2010; Morris and Western 1999). Similarly, the role of low educational qualifications in relegating individuals to the bottom of the occupational hierarchy may not have changed substantially, while earnings in occupations at the lowest class level have suffered pronounced declines (Morris and Western 1999) .
- 10. Additional analyses (available upon request) also indicate that the difference in trends does not arise from other measurement issues. First, the difference cannot be ascribed to a change from a paternal to a parental measure. Second, it is not based on a trend toward the decoupling of class background and educational background. In fact, the association between parental education and father’s class has increased over cohorts.
- 11. The finding that the compositional effect also applies to the influence of parental education on class attainment opportunities (for a direct illustration, see online supplement) also constitutes a further generalization of the compositional effect to other dimensions of socio-economic standing (Torche 2011).
- 12. The strength of the compositional effect itself could, of course, also differ across cohorts (additional analyses, available from the authors, fail to yield convincing evidence for this). This would require us to free up the COED parameter and leave us with a fully saturated model, that is, the observed trends.

Appendix

A. Decomposition Method

Breen (2010) demonstrated that the three-way probability distribution of cohort by origins by destinations (COD) can be derived from saturated log-linear models for the cross-classification of cohort by origin by education (COE) and cohort by origin by education by destination (COED), that is:

f_{ikl} = \mu \gamma_i^C \gamma_i^O \gamma_k^E \gamma_{li}^{CO} \gamma_{lk}^{CE} \gamma_{ik}^{OE} \gamma_{lik}^{COE} \tag{A1}

f_{ijkl} = \alpha \beta_l^C \beta_i^O \beta_k^E \beta_j^D \beta_{li}^{CO} \beta_{lk}^{CE} \beta_{lj}^{CD} \beta_{ik}^{OE} \beta_{ij}^{OD} \beta_{kj}^{ED} \beta_{lik}^{COE} \beta_{lij}^{COD} \beta_{lkj}^{CED} \beta_{ikj}^{OED} \beta_{likj}^{COED} . \tag{A2}

Leaving out selected, theoretically meaningful parameters from equations A1 and A2 produces counterfactual OEDC distributions, that is, predicted frequency

distributions across the four-way cross-classification of origin, education, destination, and cohort. Collapsing these counterfactual distributions over E yields an implied three-way relationship between O, D, and C, which serves as the basis for the assessment of counterfactual trends in social fluidity.

Table A1 provides an overview of the parameters included in the generation of each counterfactual COD table analyzed in this paper. The order in which we proceed differs slightly from Breen's original introduction of this decomposition method and ongoing comparative work drawing on the same modeling framework (Breen, Luijkx, and Müller forthcoming). Instead of freeing up parameters incrementally, in models 1–3 we free up a single parameter that identifies a specific mechanism in order to investigate its net effect.

Baseline Model (B)

fits parameters in both the COE and COED tables that are of no substantive interest in this paper, namely, the main effects (C, O, E, D) and all relevant two-way interactions with the exception of γ_{ik}^{CE} , that is, cohort changes in the education distribution (educational expansion). The three-way interactions that represent the main mechanisms studied in this paper (marked in gray in table A1) are only freed up in the following models.

Model 1

additionally frees up the parameters for educational expansion, γ_{ik}^{CE} , and the compositional effect, β_{ikj}^{OED} (marked in gray). It creates a simulated mobility table (COD) that allows the compositional effect to impact mobility trends through educational expansion.

Model 2

instead frees up the parameters for educational expansion, γ_{ik}^{CE} , and changes in inequality in education, γ_{ik}^{COE} . Substantively, this allows us to assess the impact of changing educational inequalities at the backdrop of educational expansion.

Model 3

frees up the parameters for educational expansion, γ_{ik}^{CE} , and changes in class returns to education, β_{ikj}^{CED} . That is, we assess the impact of changing educational returns at the backdrop of educational expansion.

Observed (O)

The last remaining three-way interaction that has not been fitted so far is β_{lij}^{COD} , that is, cohort changes in the direct effects of origins on destinations. However, fitting this parameter implies that the simulated COD cross-tabulation is equivalent to the observed COD cross-tabulation (see Breen 2010, 387). This hinders the separate assessment of the role of

cohort changes in the direct inheritance of class status outside the education system since we cannot separate it from the impact of freeing up the four-way COED interaction.

A few additional explanations may be in order:

- Note that the parameters CE and COE are always fitted in the COED table to yield consistent and unbiased parameters in the prediction of that cross-classification. However, fitting these parameters in the COED table does not require the same parameters to be fitted in the COE table. The method applied here reweights the predicted COED frequencies by the values of the fitted COE margins (personal communication with Richard Breen, November 2013).
- We add CE concurrently with the three parameters of interest in models 1–3 (rather than including CE in the baseline model) for the reasons listed below. Doing so allows us to estimate the relative impact of each mechanism separately.
 - In line with our theoretical motivation, model 1 assesses whether the compositional effect has gained a more pronounced role by virtue of educational expansion (since more individuals move into those educational positions where the OD association is lower). Freeing up OED separately would not capture this process.¹²
 - Model 2 mechanically requires fitting CE since we fit the higher-order COE interaction. Doing so, however, is also in line with our theoretical motivation that is chiefly interested in the development of educational inequalities at the backdrop of the educational expansion rather than independent of it.
 - By the same token, model 3 assesses whether educational returns have changed over time at the backdrop of the educational expansion rather than independent of it.

Appendix B

Trends for Women

Women’s labor-force participation changed radically across the cohorts studied here. As shown in table B1, it increased in our sample at a steady rate from 38 to 74 percent for the first four cohorts and remained at about that level for the last three cohorts. Given the size of our sample, this also means that we observe a

Table B1. Trends in Female Labor-Force Participation

	Year turned 30					
	Until 1951	1952–1963	1964–1975	1976–1987	1988–1999	2000–2012
% in labor force	38.4	49	60.6	73.9	75.4	73.4
Sample N in labor force	342	944	1,907	3,176	2,154	670

particularly low total count of women with occupational information in the oldest two cohorts (and, for other reasons, in the youngest cohort).

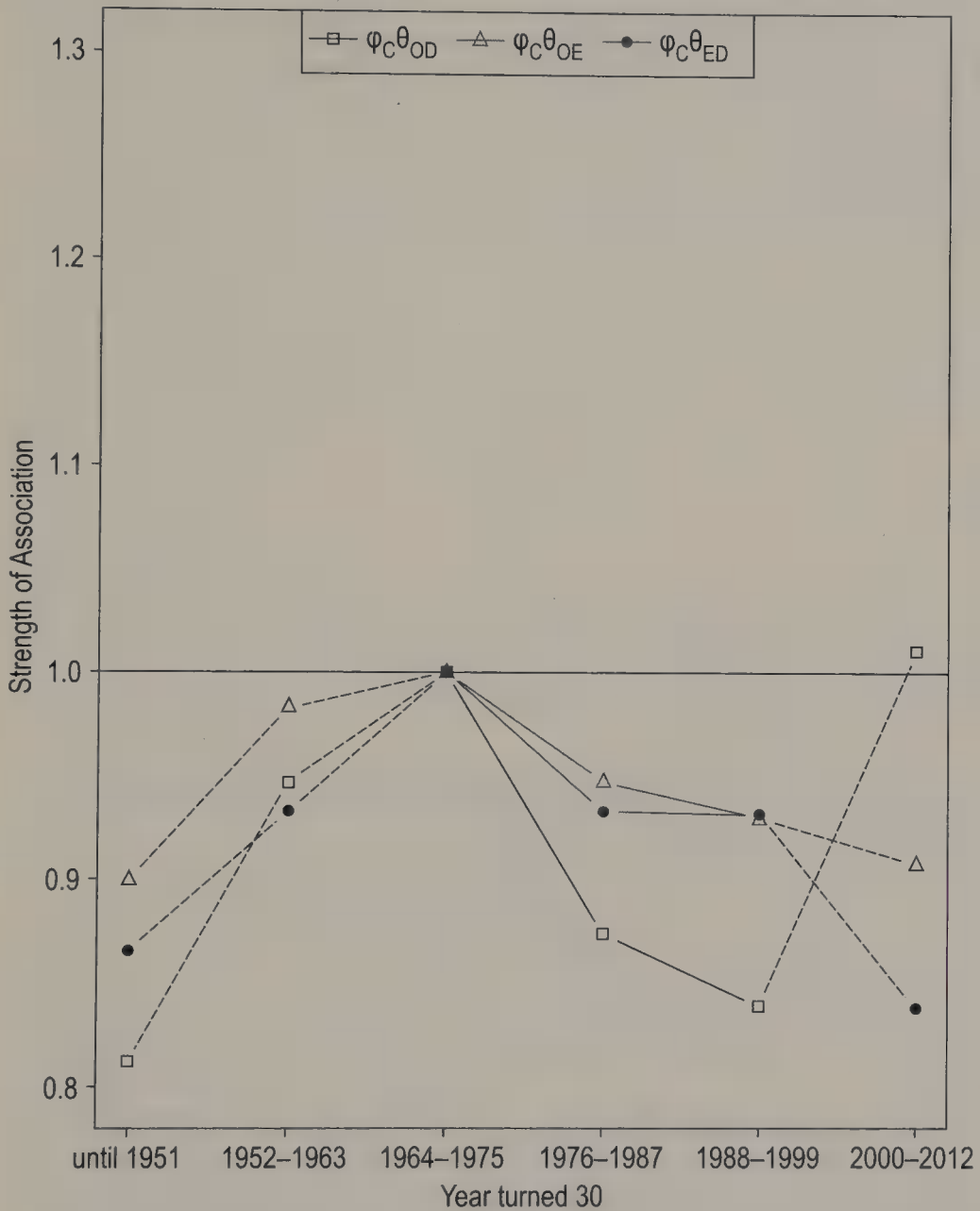
Two issues challenge the trend analysis for women. First, the low statistical power of our sample suggest that we may provide reasonable descriptive analyses for only three selected cohorts (turning 30 in 1964–1975, 1976–1987, and 1988–1999). It also prohibits us from engaging in the decomposition analysis for women not only because such analysis would be of severely limited value based on just three cohorts but also because, in even some of those cohorts, the four-way cross-classification of origin, destination, cohort, and education yields many sparse cells. Second, beyond the observed changes in the rates of female labor-force participation, we expect changes in its selectivity. That is, the factors determining whether a woman takes up work were likely much different for a woman born at the beginning of the 20th century compared to a woman born toward the end of the century. Even if we had a much larger sample, we should still consider adjustments for the expected changes in selectivity of labor-force participation. Here — as we did for men — we impute social class for all sample members based on all variables used in the analysis. Doing so is an effective strategy if values are “missing at random,” in our case meaning that selection into the labor market occurs based on the characteristics we observe (though we observe few of them, educational attainment should certainly count as an important one).

Taking both of these complications into consideration, we provide suggestive estimates to tentatively describe changes in the mobility triad for women. For the reasons just outlined, we do so with some degree of hesitation—overcome only by the importance of the scientific question—and believe that definite answers rely on efforts that expand the data basis for the assessment of mobility trends among US women (which we attempt in an ongoing project; also see Grusky, Smeeding, and Snipp [2014]). While we report results for all cohorts, we emphasize the particularly tentative nature of the estimates for the first two cohorts due to their small sample sizes and potential unobserved selectivity of labor-market participation as well as of the estimates for the last cohort, again due to its small sample size. We also run stability analyses that drop these problematic cohorts.

Whether social origins are measured as parental class or parental education and whether the analysis includes or excludes the problematic cohorts, our findings about trends in the mobility triad are the same for all legs: in all cases, the preferred model is that of constant association (results available upon request). In other words, we cannot detect statistically significant trends in social mobility, educational inequality, or returns to education for women. This finding of stability may underline the insufficiency of our data. For readers who are willing to read tendencies into the parameter estimates from the non-preferred unidiff models, we report them in figure B1. The unidiff estimates are centered on the cohort born 1934–1945, and the problematic cohorts are marked by dashed lines. The scale of the y-axis corresponds to that for men. Again, re-estimating the models restricted to the middle three cohorts yields virtually identical results. For trends based on parental class (figure B1a), we

Figure B1. UniDiff parameters (non-preferred model)

(a) Origin = Parental Class

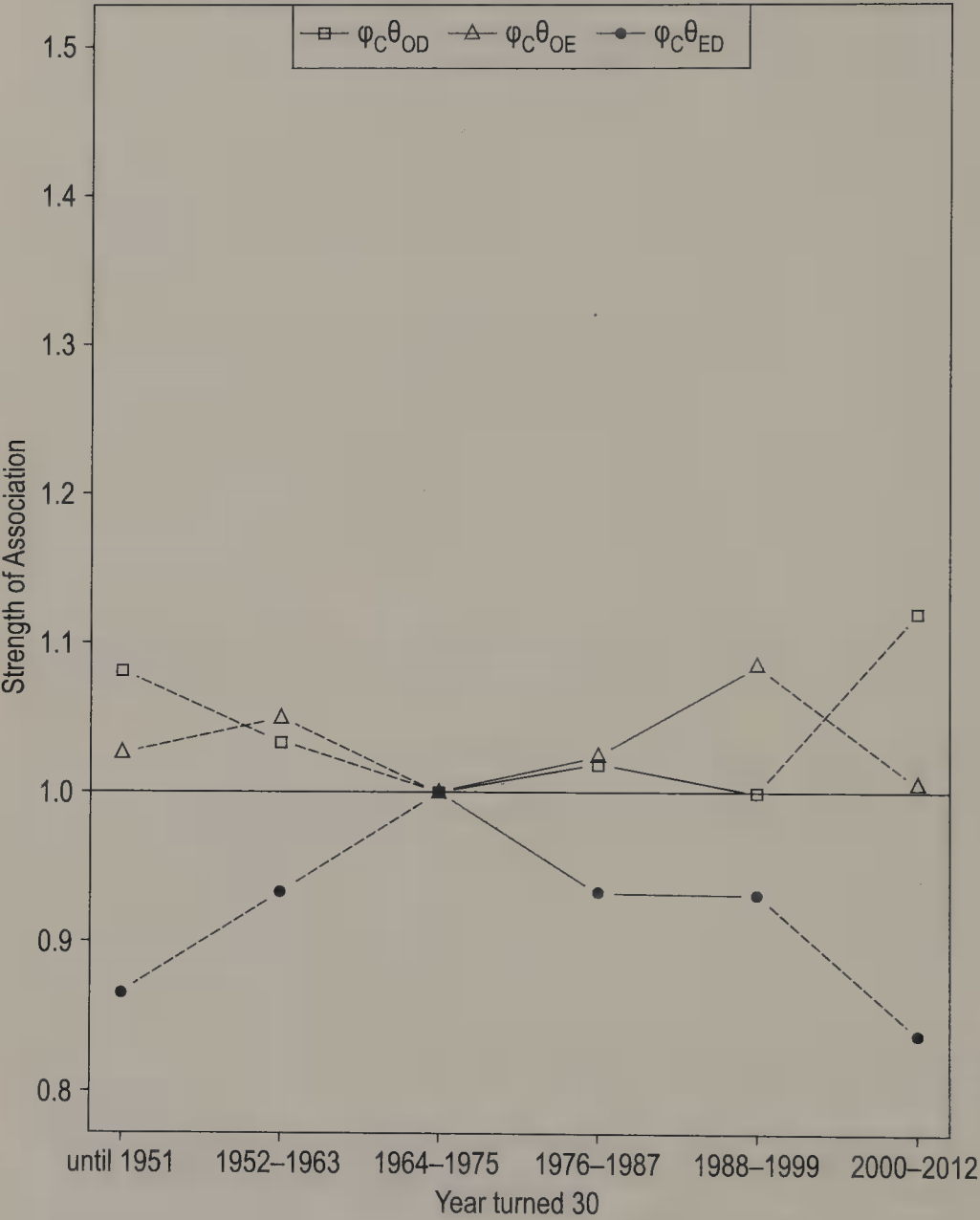


Data: GSS 1972-2012.

observe a tendency toward decreasing associations for the cohorts we consider least problematic, meaning that social mobility increases while educational inequality and returns to education decrease, but at a level that we cannot distinguish from stability based on these data. The estimates based on parental education (figure B1b) provide yet less evidence in favor of notable trends in the mobility triad for women.

Figure B1. continued

(b) Origin - Parental Education



Data: GSS 1972-2012.

About the Authors

Fabian T. Pfeffer is Research Assistant Professor at the Institute for Social Research, University of Michigan. He studies the maintenance of inequality across time and generations. Current projects focus on wealth inequality and its consequences for the next generation (*Annals of the American Academy of Political and Social Science*), the institutional context of social mobility and

educational inequality (*Social Science Research*), and the transmission of inequality across multiple generations (*Research in Social Stratification and Mobility*).

Florian R. Hertel is a PhD candidate and affiliate fellow at the Bremen Graduate School of Social Sciences and Research Associate at the Institute of Sociology, University of Bremen. He studies social mobility, inequality, and stratification from a comparative perspective. Recent publications have focused on social mobility across three generations in Germany and the United States (*Research in Social Stratification and Mobility*) and intragenerational class mobility in Germany (*Berliner Journal für Soziologie*).

Supplementary Material

Supplementary material is available at *Social Forces* online, <http://sf.oxfordjournals.org/>.

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Beyond "White by Law": Explaining the Gulf in Citizenship Acquisition between Mexican and European Immigrants, 1930

Cybelle Fox and Irene Bloemraad, *University of California–Berkeley*

Between 1790 and 1952, naturalization was reserved primarily for “free white persons.” Asian immigrants were deemed non-white and racially ineligible for citizenship by legislation and the courts. European immigrants and, importantly, Mexican immigrants were considered white by law and eligible for naturalization. Yet, few Mexicans acquired US citizenship. By 1930, only 9 percent of Mexican men had naturalized, compared to 60 percent of southern and eastern Europeans and 80 percent of northern and western Europeans. If Mexicans were legally white, why did they rarely acquire citizenship in the early decades of the 20th century? We go beyond analyses focused on formal law or individual-level determinants to underscore the importance of region and non-white social status in influencing naturalization. Using 1930 US Census microfile data, we find that while individual characteristics (e.g., length of residence and literacy) explain some of the gulf in citizenship, the context of reception mattered nearly as much. Even if Mexicans were “white by law,” they were often judged non-white in practice, which significantly decreased their likelihood of naturalizing. Moreover, the more welcoming political and social climate of the Northeast and Midwest, where most European migrants lived, facilitated their acquisition of American citizenship.

Introduction

Scholars of race, ethnicity, immigration, and citizenship have long studied the role of US law and courts in structuring racialized notions of membership. Racial exclusions to citizenship were written into the first Naturalization Act of 1790; in order to naturalize, individuals had to be “free white persons.” Following the Civil War, the Naturalization Act of 1870 broadened this provision to encompass “aliens of African nativity and to persons of African descent.” But racial

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exclusions to naturalization were eliminated definitively only with the Immigration and Nationality Act of 1952.¹

Absent from early laws was any mention of people understood to fall outside the categories of “white” or “African descent,” or the precise boundaries of the “white” population. This legal silence grew problematic as migration from Asia grew in the latter half of the 19th century. The courts consequently attempted to specify which groups were “white by law.” Through a series of intellectually inconsistent judicial decisions that variously called on “scientific” notions of race and color, the classification strategies of the “average man,” congressional intent, and geographic origins, immigrants of East and South Asian origins were deemed non-white and therefore ineligible for naturalization. These laws and court decisions had lasting consequences on notions of race, deservingness, and membership, and reinforced power hierarchies and subordination by structuring access to everything from business licenses to suffrage rights (Gross 2008; Haney López 1996; Smith 1997).

We go beyond formal law to understand historical processes of citizenship acquisition for other racialized subjects. We do so by highlighting an understudied group within this literature: Mexican immigrants. Like Europeans, the courts deemed Mexicans legally eligible for naturalization. Yet, in stark contrast to Europeans, including southern and eastern Europeans, very few Mexican migrants acquired US citizenship. By 1930, only 9 percent of Mexican men had naturalized, compared to 60 percent of southern and eastern Europeans and 80 percent of northern and western Europeans.²

If Mexicans were white by law, what explains the gulf in citizenship acquisition between Mexican and European immigrants? Traditional accounts of citizenship acquisition among European immigrants focus on individual-level differences between people and groups. Key factors explaining why these immigrants became “Americans by choice” (Gavit 1922) include time in the United States, socioeconomic status, or an individual’s intent to return to his or her country of origin, what we might call differences in resources, skills, and motivations (Bernard 1936; Gavit 1922; Gosnell 1928, 1929; see Bloemraad [2006a] for one exception).

This paper supplements, and in some cases challenges, the attention to either law or individual attributes to explain historical naturalization. Instead, we draw attention to variation in the treatment experienced by different immigrants living in different parts of the United States. Scholars of contemporary immigration note that integration experiences are often influenced by the “context of reception,” a term that can encompass diverse structural and social processes. Much of this literature highlights the fact that immigrant incorporation varies across geographic space. Immigrant incorporation may vary cross-nationally due to labor-market structures and social policy (e.g., Crul and Schneider 2010; Kesler 2006; Reitz 1998), or with citizenship and multiculturalism policy (e.g., Bloemraad 2006b; Koopmans et al. 2005). Within the United States, geographic contexts of reception affect contemporary migrants who move to new gateway cities, suburbs, or rural areas outside traditional gateways (e.g., Singer, Hardwick, and Brettell 2008), or to the South, a region with little immigration experience and a history of segregated race relations (e.g., Massey 2008; Marrow 2011).

The concept of context of reception has also been used, however, to distinguish outcomes between contemporary immigrant groups within the same location. Depending on their racial status, different groups may face hostile, welcoming, or neutral receptions, which may generate distinct assimilation trajectories (Portes and Böröcz 1989; Portes and Rumbaut 2001; Portes and Zhou 1993).

We extend the concept of context of reception—attentive to both regional variation and color status—to explain differences in naturalization between Mexican and other migrants in the first third of the 20th century. Canadians enter our analysis to show how living in a border country affected naturalization, but we focus our attention on the Mexican-European comparison. Some scholars suggest that the context of reception faced by European immigrants, especially those from southern and eastern Europe in the early 20th century, was similar to the context of reception that Mexicans face today (Alba 2009; Perlmann 2005). We instead compare Mexican and European immigrants at the same historical moment. We argue that while variation in resources, skills, and motivations helps explain some of the gulf in citizenship acquisition, it is insufficient to explain it all. Non-white social status mattered. Even though Mexicans were white by law, they were usually judged non-white in practice. We find that non-white social status significantly decreased Mexicans' likelihood of naturalizing. Regional context also mattered, but primarily for European immigrants. The more welcoming political and social climate of the Northeast and Midwest facilitated their acquisition of citizenship. Since this is where most European immigrants lived, this deepened the national citizenship gulf with Mexicans.

To make our case, we evaluate existing historical scholarship and undertake an original empirical analysis using a 5 percent sample of the 1930 US Census. The 1930 Census microfile data include large numbers of European, Canadian, and Mexican immigrants and individual-level measures of resources, skills, and motivations, as well as geographic identifiers to evaluate regional variation in context of reception. We also take advantage of a novel enumeration procedure in the 1930 Census, one that classified most *but not all* Mexicans as non-white, allowing us to estimate more precisely the effect of a non-white social status on the propensity to naturalize.

White by Law

Between 1790 and 1952, judges were called upon to define the boundaries of whiteness as a precondition for naturalization in 52 racial prerequisite cases. The courts ultimately found that Asian immigrants were non-white.³ By contrast, European immigrants were judged to be white and eligible for citizenship. In fact, aside from immigrants from the borderlands between Asia and Europe—especially Syrians and Armenians—the color status of European immigrants was rarely litigated (Gross 2008). US naturalization attorneys in Minnesota did attempt to bar some politically “radical Finns from naturalizing on the ethnological grounds that they were ‘Mongolian’ and therefore not white,” but the judges hearing the case affirmed their claims to whiteness, stating that the Finns “are now among the whitest people in Europe” (Roediger 2005, 61). Indeed, in its landmark *US v. Thind* decision in 1923, which declared Asian Indians ineligible

for naturalization because they were not white by the standards of “the great body of our people,” the Supreme Court justices characterized “immigrants from Eastern, Southern and Middle Europe” as “unquestionably akin” to northern and western Europeans, all belonging to “the various groups of persons in this country commonly recognized as white” (Haney López 1996, 182). Many scholars underscore the consequential and sharp membership divide created by the law and courts, which opened a wide door to naturalization for European migrants, regardless of religion and property, but which firmly shut that door to Asian migrants (Ueda 1982; Zolberg 2006).

The situation of Mexican migrants adds an important nuance to a simple story of legal inclusion or exclusion. By law, Mexicans were eligible for naturalization. But unlike European immigrants, their eligibility was a product of foreign relations and treaties rather than any “scientific” or common acceptance of their whiteness (Fox and Guglielmo 2012). The Treaty of Guadalupe Hidalgo (1848), which transferred large swaths of land from Mexico to the United States, extended US citizenship to Mexicans in the newly incorporated territories. When Ricardo Rodríguez’s 1893 application to become a US citizen was challenged on the grounds that he was not white, the court upheld Rodríguez’s application because of US treaties with Mexico. But the court also noted that “if the strict scientific classification of the anthropologist should be adopted, he would probably not be classed as white” (Foley 2004, 345).

The Rodríguez decision served as an important precedent in future cases when Mexicans’ whiteness was on trial. By granting Mexicans naturalization at a time when naturalization was reserved for free white persons and persons of African descent, the court was treating Mexicans “as though they were white” (Gross 2008, 259). When state courts subsequently enforced laws banning marriages between blacks and whites, they “began with the presumption that Mexican Americans were white” (Gross 2008, 260). In this sense, Mexicans were “white by law.”

This claim to legal whiteness, however, was tenuous since it did not align well with scientific or popular ideas about Mexicans’ color status. Those overseeing naturalization continued to question whether Mexicans were racially eligible for citizenship (Molina 2010, 178–79). Under pressure from a California nativist organization, a New York judge upheld an immigration officer’s denial of the naturalization petitions of three Mexicans in 1935 because they were not white, but rather individuals “of Indian and Spanish blood” (Lukens 2012, 121). Had it been sustained on appeal, this decision could have made most Mexicans ineligible for citizenship (Molina 2010). Concerned about how such a decision would affect US-Mexico relations, the US State Department tried to “quiet the controversy,” convincing the judge to reverse his decision. Labor Department officials warned field officers “to withdraw all appeals based on race,” and ordered that “in all future cases, [Mexican] immigrants be classified as ‘white’” (as quoted in Fox 2012, 44; Lukens 2012). To prevent further controversy, Congress amended its naturalization laws in 1940 to include “all races indigenous to the Western Hemisphere” (Lukens 2012; Molina 2010). Yet, Mexicans’ claims to whiteness were not evenly recognized, and their color status was far from settled (Fox and Guglielmo 2012).

Resources, Skills, and Motivations

If Mexicans were legally white and eligible for naturalization, can their low levels of citizenship be understood using prevailing explanations applied to European immigrants? A major concern at the time was that limited citizenship among newer southern and eastern European immigrants, relative to earlier northern and western European migrants, reflected biological and cultural differences, an idea advanced by the Immigration (Dillingham) Commission of 1911 (Gavit 1922; Ueda 1982). This framing, pitting European "races" against each other, set off the first academic debate over naturalization as critics amassed empirical evidence to challenge charges of racial inferiority (DeSipio 1987). John Palmer Gavit (1922) culled through more than 26,000 naturalization petitions in 1913–14, ultimately concluding that citizenship differences were largely attributable to immigrants' length of residence, not inherent racial, cultural, or economic differences. Drawing on a survey of 3,500 immigrants in Chicago, Harold Gosnell emphasized immigrants' motivations and perceptions of the benefits of citizenship, including a desire to identify with the community, secure economic advantages, and gain the vote (1928, 938). William Bernard contended, based on a survey of immigrants in New Haven, that naturalization was largely driven by education, occupational status, and income (1936, 948, 953). Later multivariate statistical analyses of eight European national-origin groups using 1900 and 1920 Census microfile data confirmed the importance of length of residence, literacy, occupation, and English ability (Bloemraad 2006a). In short, variation in naturalization among European immigrants flowed from differential resources, skills, and motivations, a perspective that continues to the present.

Applied to Mexican migrants in the early 20th century, this approach suggests that their low naturalization levels stem primarily from individual characteristics. For example, length of residence might play a determinative role (Grebler 1966). Mexican immigration started to increase significantly only during World War I, more than two decades after the beginning of mass immigration from southern and eastern Europe. In 1930, Mexican men had lived in the United States an average of 18 years, compared to 23 years for southern and eastern Europeans and 33 years for northern and western Europeans (see table 1).

Individual skills and resources might also matter. The Naturalization Act of 1906 reiterated many of the requirements of earlier laws: a mandatory residency period (five years), good moral character, and an oath of allegiance. Would-be citizens also had to convince a judge of their ability to speak English and their knowledge of the US Constitution. Many contemporaries suggested that low literacy and poor English partly explained low Mexican naturalization (Krichefsky 1963; US Immigration Commission 1911; Walker 1929). In 1930, only 71 percent of Mexican men were literate, compared to 87 percent of southern and eastern Europeans and 98 percent of northern and western Europeans. Census enumerators reported that only 53 percent of Mexicans spoke English, compared to more than 90 percent of Europeans.

The naturalization process also entailed financial costs, equivalent to two or three days' pay for some applicants (Reisler 1976; Rich 1940). Non-citizens

Table 1. Descriptive Statistics, Immigrant Men, 21 Years and over, 1930

	Mexicans	Southern & eastern Europeans	Northern & western Europeans	All Canadians
Naturalized citizen (%)	9	60	80	65
Resources, skills and motivations				
Time in US (yrs)	18	23	33	31
Speaks English (%)	53	93	98	98
Literate (%)	71	87	98	96
SEI score ^a (0–96)	13	25	24	28
Age at migration (yrs)	21	20	19	19
Own a home (%)	22	46	55	48
Married, spouse present (%)	62	73	67	70
Married, spouse absent (%)	5	5	3	4
Children in household (%)	55	66	54	54
From border country (%)	100	0	0	100
Served in WWI (%)	1	6	5	7
Racism				
Classified as “white” (%)	4	100	100	100
Region				
Northeast & Midwest (%)	9	87	75	80
South (%)	1	3	3	2
Southwest (%)	82	6	9	8
Northwest (%)	8	4	13	10
Observations	11,936	165,548	122,945	23,976

^aCalculated for those in the labor force only.

Source: IPUMS, 5% Census sample, 1930.

frequently cited cost to explain why they had not yet naturalized (Schneider 2001, 58). Although the Census Bureau did not collect information on income prior to 1940, Mexicans’ lower incomes (and modest educational attainment) can be inferred by their scores on the Duncan Socioeconomic Index (SEI), which was only 13 for Mexicans (similar to the score for black Americans), compared to 25 for southern and eastern Europeans and 24 for northern and western Europeans.

Another possible determinant is veteran status. During World War I, citizens and non-citizens were subject to the draft. In 1918, Congress amended the law to facilitate citizenship for aliens who served in the armed forces. The law eliminated the need for first papers, proof of long-term residence, background checks, and sometimes even knowledge of English or the US Constitution, and it reasigned examiners from civilian courts to military bases (Gavit 1922, 255–65;

Salyer 2004; Schneider 2011, 212–13). All told, 288,000 military men took the oath of allegiance, representing “almost a quarter of all those naturalized during the years 1918 to 1924” (Schneider 2011, 213). But Mexicans (1 percent) were less likely than southern and eastern European (6 percent) or northern and western European (5 percent) immigrants to serve in the US military during World War I.

Many contemporaries also argued that Mexicans held distinct motivations for migration. Commentators underscored that Mexican migrants were “not interested in becoming naturalized” because they intended to return to Mexico after some years of work (Walker 1929, 466; see also Bogardus 1930; US Immigration Commission 1911). While we lack systematic data on individuals’ intent to return, we can gain traction on the question with Census data. For example, individuals who owned a home in the United States may have had “stronger ‘roots’ in local communities” and envisioned a permanent stay (Portes and Curtis 1987, 361). Only 22 percent of Mexicans owned a home in 1930, compared to 46 percent of southern and eastern Europeans and 55 percent of northern and western Europeans.

Other measures, however, ones less dependent on wealth, cast doubt on the idea that Mexicans naturalized at lower rates simply because they intended to return to Mexico. A high proportion of male migrants could indicate a sojourner mentality—with migrants planning to return to their families in the homeland after accumulating savings—but we find only slight differences in gender ratios: men made up 56 percent of all Mexicans, 56 percent of southern and eastern Europeans, and 52 percent of northern and western Europeans. Similarly, immigrants who were married but whose spouses were still in the country of origin might be more likely to return, while those with families in the United States might be more likely to stay. While southern and eastern Europeans were somewhat more likely than Mexicans to be married with a spouse present in the household (73 versus 62 percent, respectively), and to have children (66 versus 55 percent, respectively), there were fewer differences between Mexicans and northern and western Europeans in this regard.

Proximity to their homeland might also have fostered dreams of return and depressed naturalization among Mexicans (Garcia 1981). Migrating from a border country could make the process of naturalization more difficult, too. Immigrants who arrived “in or after 1906 had to produce a Certificate of Arrival issued by the Bureau of Immigration (which kept a list of arriving passengers) in order to receive their first papers” (Schneider 2011, 206). But “getting such a certificate turned out to be a significant problem for many immigrants—especially for those who had crossed land borders—because the official records were incomplete, faulty or simply nonexistent” (Schneider 2011, 206). Canadian immigrants’ experiences are instructive here, as historical scholarship suggests similar dreams of return, at times encouraged by institutions and elites in Canada, as well as bureaucratic difficulties in securing official records of entry (Ramirez 2001; Gabaccia 2007). Yet, although Canadians were also from a border country, their naturalization level was seven times higher than that of Mexicans (65 versus 9 percent, respectively).

The Context of Reception

A desire to return to one's homeland can also be influenced by the context of reception. If one does not feel welcome, one may dream of leaving (Humphrey 1944; Portes and Curtis 1987; Reisler 1976, 114–15). A group's racial or color status and the level of discrimination they encounter is part of the national context of reception. As Portes and Rumbaut argue, "Though race is in appearance a personal trait, in reality it inheres in the values and prejudices of the culture... In America, race is a paramount criterion of social acceptance" (2001, 47). Geographically based differences in native-born groups' efforts at civic and political incorporation also create a regional context of reception. We argue that existing scholarship has paid insufficient attention to the effects of non-white social status and geographical contexts on historical naturalization.

National Context of Reception: White by Law, Not in Practice

Whiteness studies scholars have popularized the notion that southern and eastern European immigrants were not considered white when they arrived in large numbers in the 1890s. Rather, they had to "work toward whiteness," and achieved it securely only after the immigration restrictions of the 1920s, or perhaps as late as World War II (Barrett and Roediger 1997; Brodtkin 2000; Roediger 2005). Other scholars, however, suggest that such claims are exaggerated, even while they recognize that southern and eastern Europeans suffered more discrimination than their northern and western European counterparts (Arnesen 2001; Fox and Guglielmo 2012; Gross 2008; Guglielmo 2003). Both sets of scholars would agree that discrimination might have depressed naturalization among southern and eastern Europeans relative to northern and western Europeans, consistent with aggregate levels of naturalization in 1930, 60 versus 80 percent, respectively. But the latter group of scholars would argue that color status had little to do with it. Guglielmo (2003) argues that discrimination against southern and eastern Europeans was usually centered on religion, nationality, citizenship, or race—not color status. These migrants, he claims, were "white on arrival."

The key distinction lies between the concepts of "race" and "color" (see Jacobson 1998, 5–7). In the early 20th century, color was often used to refer to groups like whites or blacks, what the Dillingham Commission called the "grand divisions of mankind" (US Immigration Commission 1911, 6). Race described these categories as well, but *also* smaller ones like Italians, Germans, Nordics, and Jews. Italians were widely considered racially inferior to the French, but it was not because they were perceived to be non-white. When Italians filled out their Declaration of Intent, the first step in the process of naturalization, they listed their color as "white," and their race as northern or southern "Italian." However racially inferior southern and eastern European immigrants were deemed to be, they were treated by most American institutions—including the courts, the Census, political parties, unions, schools, realtors, and social workers—as white (Fox and Guglielmo 2012; Guglielmo 2003).

Mexicans, however, straddled the boundaries of whiteness (Foley 2004; Fox and Guglielmo 2012; Guglielmo 2006). Mexicans were white by law, but they were often

perceived to be non-white by race scientists, local, state and federal officials, as well as the "common man." This determination of non-white social status could be based on how others read physical attributes (skin color, hair, etc.) as well as language, surname, or socio-economic condition. Such social designation was consequential. Unlike European immigrants, Mexicans often attended segregated schools, lived in segregated neighborhoods, and were excluded from public accommodations on the basis that they were Mexican and therefore not white (Foley 1997, 2004; Fox and Guglielmo 2012; Haney López 1998; Reisler 1976 Valdés 2000).

Mexicans' non-white social status might have affected naturalization in two key ways. First, Mexicans who applied for naturalization may have faced more discrimination than Europeans because examiners questioned Mexicans' color status or subjected them to different standards. Contemporaries argued that there were few common standards to evaluate suitability for citizenship (Rich 1940). Knowledge of English was, according to one observer, "enforced with a great variety of degrees of strictness" (Gavit 1922, 120). Similarly, there were no national rules for how to evaluate "knowledge of the constitution" (Schneider 2001, 57), rendering the examination process "subject to the whims, theories, prejudices, and intellectual limitations of the individuals upon whom its enforcement devolves" (Gavit 1922, 123). Menchaca's examination of naturalization applications filed by Mexican and European migrants in South Texas between 1848 and 1906 is consistent with a discrimination argument. She found that only 1.4 percent of Mexicans who applied for citizenship were successful, compared to 44 percent of Europeans (2011, 256–58).

Second, Mexicans' non-white social status might have affected their interest in applying for citizenship. According to Mexican sociologist Manuel Gamio, European immigrants were more eager to naturalize because they were more socially accepted. For white immigrants, "There is no racial prejudice to keep him from intermarriage with Americans," and they are "more often able to reach a position of economic parity with native-born Americans." But for Mexicans, even after naturalization, "the racial and other prejudice against him continues, and his social and economic conditions are scarcely changed" (1971[1930], 128). "What is the use?" explained one Mexican migrant. "They will call me a dirty greaser anyway" (Bogardus 1930, 78). Under these circumstances, the benefits of US citizenship were modest. Furthermore, there were advantages to retaining Mexican citizenship, including appealing to the Mexican Consul when faced with discrimination (Bogardus 1930; Sánchez 1993). Menchaca argues that Mexicans in Texas applied for naturalization at a much lower rate after the turn of the century as anti-Mexican sentiment increased and efforts to disenfranchise Mexican Americans became more widespread and effective (2011, 162–63, 179–80).

To measure the effect of non-white social status on naturalization, we turn to a unique feature of 1930 Census data. For decades, Census enumerators were tasked with gathering information on the nation's inhabitants, including residents' country of birth—which allows us to classify migrants by national origin—as well as their "race or color." Virtually all immigrants from Europe were classified as "white" in every US Census. Up until 1920, most individuals from Mexico were classified as "white" as well. But in 1930, responding to political pressure from Congress, the Census Bureau distinguished whites from a new "Mexican" "color or race" status

(Hochschild and Powell 2008). The Census instructed enumerators: “All persons born in Mexico, or having parents born in Mexico, *who are not definitely white*, Negro, Indian, Chinese, or Japanese, should be returned as Mexican” (US Bureau of the Census 1933, 27, italics added). The Census Bureau acknowledged the messiness of race and color status and did not provide enumerators with specific instructions to determine who was or was not “definitely white,” but they hinted that local custom might be a reasonable guide: “practically all Mexican laborers are of a racial mixture difficult to classify, though usually well recognized in the localities where they are found” (US Bureau of the Census 1933, 27). The boundary between white and non-white social status might consequently vary somewhat across localities. Census enumerators appear to have employed a narrow definition of whiteness, recording only 3.6 percent of immigrants from Mexico as white in 1930; the rest were categorized as racially Mexican.

Cross-tabulations of Census data by birthplace and “race or color” show that white Mexican men were more than three times as likely to be naturalized as non-white Mexican men (27 versus 8 percent, respectively; see table 2). Mexicans

Table 2. Descriptive Statistics, Mexican Men, 21 Years and over, 1930

	White Mexican	Non-white Mexican
Naturalization rates (%)	27	8
Resources, skills, and motivations		
Time in US (yrs)	19	18
Speaks English (%)	84	52
Literate (%)	89	71
SEI score ^a (0–96)	23	13
Age at migration (yrs)	18	21
Own a home (%)	25	22
Married, spouse present (%)	48	62
Married, spouse absent (%)	5	5
Children in household (%)	38	55
Served in WWI (%)	4	1
From border country (%)	100	100
Racism		
Classified as “white” (%)	100	0
Region		
Northeast & Midwest (%)	34	8
South (%)	5	0.5
Southwest (%)	54	83
Northwest (%)	7	8
Observations	431	11,505

^aCalculated for those in the labor force only.

Source: IPUMS, 5% Census sample, 1930.

categorized as white were also more likely to speak English, to be literate, to have higher socioeconomic status, and to own a home. It is possible that their higher socioeconomic status followed from lighter phenotype, since darker Mexicans suffered more discrimination (Foley 1997, 41; Gamio 1971[1930], 53). Alternatively, English ability, homeownership, literacy, and higher socioeconomic status might have “whitened” some Mexicans in the eyes of locals and enumerators. This possibility was recognized by contemporaries. Writing in the *LULAC News*, the President General of the League of United Latin American Citizens discussed the “ticklish question” of how enumerators determined who was racially “Mexican,” which could be based on whether “The subject speaks Spanish, or has a Mexican (meaning Spanish) name, or looks ‘Mexican,’ or is dark, or...[is] in destitute circumstances, and is illiterate, or is a day laborer, a cotton picker or a beetfield worker” (Salinas 1939, 7). Irrespective of whether phenotype and other markers drove socio-economic outcomes, or whether they were read in light of education, occupation, and wealth, if non-white social status was a deterrent to naturalization, we would expect that individuals classed as “white”—including white Mexican immigrants—would be more likely to naturalize than those who were not, even controlling for individual resources.

More problematic for our analysis is the possibility that Census enumerators’ evaluation of color status was dependent on their knowledge of an individual’s citizenship status. If American citizenship might “whiten” Mexicans, then the causal arrow we propose, namely that color status affects naturalization, may run in the opposite direction. But there are good reasons to believe that citizenship status, by itself, had very limited influence on race or color classifications. As a technical matter, when filling out the Census form, enumerators had to note racial or color status ahead of the question on birthplace and citizenship. Conceptually, just as African Americans could be classified as non-white and US citizens, so could those of Mexican origin. Indeed, 96 percent of second-generation Mexican Americans—all of whom held US citizenship based on birthplace—were classified as non-white Mexicans in the 1930 Census. Finally, when discussing the classification of Mexicans as non-white in the Census, the President General of LULAC explained how high socioeconomic status might “whiten” Mexicans, but did not mention that American citizenship could do the same. In fact, he noted that a “bona fide, real Mexican citizen” might be classified as racially “white” instead of “Mexican” (Salinas 1939, 7).

Regional Differences in the Context of Reception

Low Mexican naturalization may have also been influenced by regional context. In 1930, 82 percent of Mexican immigrants lived in the Southwest, while 87 percent of southern and eastern Europeans and 75 percent of northern and western Europeans lived in the Northeast and Midwest. Naturalization levels varied significantly by region: 68 percent of foreign-born men in the Northeast and Midwest were naturalized, compared to only 47 percent in the Southwest. These differences are not simply a function of the larger proportion of Mexicans in the Southwest; European naturalization levels were also somewhat depressed there.

These regions were distinct in many ways that may have affected naturalization levels, from geography, population density, and demography to political

economies, local histories of race relations, “Americanization” efforts, and political context (Fox 2012). Given the importance of political context and Americanization efforts for naturalization, we focus on these two factors to illustrate how region could matter.⁴

Regional Variation in Political Context

Early in the 20th century, Northeastern and Midwestern cities were rife with machine politics. In many cities, political machines provided services for immigrants in their wards, including direct assistance with naturalization (Forthal 1948; Schneider 2001, 54; Stave 1970). Party workers could fill out naturalization papers, act as witnesses, teach immigrants English, and coach them for the citizenship examination. In addition, a party worker might speak “to the naturalization examiner” on “behalf of the declarant,” and pay his fees (Forthal 1948, 38). In 1928, more than 70 percent of Chicago’s Democratic precinct captains assisted “their constituents with naturalization” (Erie 1988, 94). Party workers elsewhere often did the same (Erie 1988, 94–95; Gosnell 1928, 937).

In the Southwest, by contrast, municipal reformers were more successful in repelling machine politics. They saw that machines drew their strength “from the unquestioning partisanship of working class, especially immigrant, voters bought with the dispensation of favors and patronage.” Thus, they advocated at-large, nonpartisan elections to “erode the ties of party,” and instead promoted government by the city’s leading experts (Bridges 1997, 7–8). Cities in the Southwest adopted, on average, 2.5 out of these three municipal reforms, compared to only 1.6 in the North (Fox 2012, 46).

Municipal reformers did little to encourage political participation (Sonenshein 2006, 27). In fact, they often allied with nativists or eugenicists and shared these groups’ “antipathy to immigrants and people of color” (Bridges and Kronick 1999, 693). Wishing to limit suffrage to “worthy” voters, municipal reformers championed literacy testing, poll taxes, early registration, and longer residency periods for voting (Bridges 1997, 8). Even where machines existed, especially in rural areas of Texas, scholars find no evidence that they did much to encourage Mexican naturalization in the early 20th century. Aliens were eligible to vote in Texas until 1921, so political bosses dependent on the Mexican vote needed only to arrange for aliens to declare their intention to naturalize (Anders 1982, 16, 250). Moreover, the Ku Klux Klan, women’s suffrage groups, prohibitionists, and municipal reformers made voting increasingly difficult for Mexicans and Mexican Americans throughout Texas in the first third of the 20th century (Menchaca 2011). The result was low voter participation, little electoral competition, and the disfranchisement of much of the naturalized immigrant community throughout the Southwest (Bridges 1997; Fox 2012).

Regional Variation in Americanization Efforts

There were also regional differences in efforts to “Americanize” immigrants. The Americanization movement, which began at the turn of the 20th century, took on new significance during World War I as concerns about national solidarity intensified (Higham 1994[1955]; King 2000). Many aspects of this movement

were coercive. Initially, the “100 percent Americanism” movement targeted Germans, who were forced to abandon their language, newspapers, and German schools. The targets of nativism soon broadened: some businesses promoted only employees who were citizens or who declared their intention to do so (Leiserson 1924, 249–53; Roediger 2005, 208–9). In 1916, Congress threatened to deport all immigrants who refused to naturalize within three months of becoming eligible. The proposal did not pass, but the number of deportations increased significantly (Higham 1994[1955], 248–49, 255).

However, institutions involved in Americanization also promoted more benign methods to encourage naturalization. The US Board of Education established a Division of Immigrant Education, which funded English language, literacy, and civics instruction. The US Bureau of Naturalization promoted immigrants' civic education, cooperating with school officials in 2,000 communities to ensure that they had lists of immigrant children subject to compulsory attendance laws as well as the names of adults who might benefit from citizenship classes (Higham 1994[1955], 242; King 2000, 87–120; Schneider 2001; Thompson 1920, 46–48).

We find little variation in *coercive* Americanization between regions. Following World War I, some state legislatures passed laws requiring citizenship for certain professions or union membership (Vernier 1938). Southwestern, Northeastern, and Midwestern states passed such laws at similar rates. There were also no significant regional differences in state Americanization laws, which required that schools use English as a medium of instruction, teach American history and government, place American flags on schoolhouses, and hold patriotic exercises (King 2000, 114).

In contrast, more inclusionary Americanization efforts varied significantly by region. Northeastern and Midwestern state legislatures were more likely to pass legislation favoring adult English and citizenship classes (83 percent) than Southwestern states (50 percent) (US Bureau of Education 1925). Similarly, in 1927–28, Southwestern states devoted only \$0.22 per alien resident on naturalization education, compared to \$1.42 in the Northeast and Midwest (US Bureau of Education 1929). Furthermore, while other institutions, including churches, settlement houses, and employers, worked to incorporate European immigrants in the Midwest and Northeast, these institutions did less to Americanize Mexicans, regardless of where they lived (Fox 2012). There were also few social service agencies along the Mexican border, and those that did exist often refused to serve Mexicans (Hanna 1935). The relative dearth of benign Americanization services in the Southwest reflects in part the deep ambivalence that native-born whites felt about whether to encourage the political incorporation of Mexicans (Reisler 1976; Sánchez 1993; Valdés 2000).

Regional differences in the warmth of reception may have affected all immigrants equally, or regional differences may have mattered more for some groups than others. If Mexicans in the Northeast and Midwest were ignored by political machines or Americanization efforts due to overriding social exclusion based on color, then regional differences should matter less for them. Conversely, Europeans' white color status, irrespective of residence, suggests that region-specific dynamics could affect them more. We thus consider whether the impact of regional residence on naturalization varied across groups.

Data and Models

To test these possible explanations, we use a 5 percent sample of the 1930 Census from the Integrated Public Use Micro Data Sample (IPUMS) (Ruggles et al. 2010). We restrict our sample to foreign-born men, 21 and over, who had entered the country at least five years prior to 1930, since these were requirements of naturalization. We exclude women because their naturalization was tightly linked to that of their husbands, especially before 1922 (Bredbenner 1998). Although we are primarily interested in understanding the naturalization gap between Mexican and European immigrants, we include Canadians in our models to test whether low naturalization among Mexicans is the result of living in a border country. We are left with a data set containing 11,936 Mexican, 165,548 southern and eastern European, 122,945 northern and western European, and 23,976 Canadian immigrant men.

Our dependent variable is self-reported citizenship status,⁵ coded 1 if the immigrant is a naturalized US citizen and 0 if he is not. We include dummy variables for birth in Mexico, in southern or eastern Europe, or French Canada; our reference group includes those from northern or western Europe or English Canada.⁶ These variables aim to capture the crucial distinction of the time between “older” and “newer” European migrants, they incorporate our key comparison with Mexican migrants, and they allow us to include Canadians as a group of border immigrants. We differentiate between French and English Canadians since the two were usually set apart by language and religion, with (Catholic) French Canadians experiencing more discrimination than their Anglophone (and usually Protestant) compatriots (Gabaccia 2007; Ramirez 2001; Richard 2009) (see appendix B for descriptive data on English and French Canadians).

To evaluate whether the context of reception is consequential for naturalization, we employ a second measure, dividing Mexicans into two groups: those who were classified as white by Census enumerators and those classified as non-white. Regional variation is captured by dummy variables for South, Southwest, and Northwest residence. The reference group is Northeast and Midwest.

While our central interest lies in racial and regional contexts of reception, the primary alternative account of naturalization outcomes centers on individuals' resources, skills, and motivations. We therefore include continuous variables for age at migration, years since migration, and SEI. We include dummy variables measuring literacy, ability to speak English, and WWI veteran status. We also include dummy variables for homeownership, marriage with a spouse present in the household, marriage with a spouse absent, having children in the household, or being born in a country that borders the United States (i.e., Canada or Mexico).

Results

We start with the bivariate relationship that motivated this research: variation in naturalization by birthplace (see table 3). We employ a logit regression model with robust standard errors adjusted for the fact that the data are clustered in

Table 3. Determinants of Naturalization for Foreign-Born Men, 21 Years and over, 1930

VARIABLES	(1)	(2)
	National origin	Resources, skills, and motivations
Mexican	-3.628*** (0.215)	-1.632*** (0.251)
Southern & eastern European	-0.871*** (0.106)	-0.441*** (0.087)
French Canadian	-0.981*** (0.130)	-0.377*** (0.090)
Years in USA		0.071*** (0.003)
Age at migration		-0.009*** (0.001)
Literate		0.962*** (0.070)
Speaks English		1.624*** (0.082)
SEI score		0.016*** (0.001)
Owens home		0.296*** (0.049)
Children		0.153*** (0.023)
Married, spouse present		0.320*** (0.023)
Married, spouse absent		-0.117** (0.051)
Border country		-0.780*** (0.077)
Served in WWI		1.372*** (0.050)
Constant	1.295*** (0.085)	-3.805*** (0.096)
Observations	324,863	324,863

Note: Robust standard errors in brackets. The reference group for origin is English Canadians and northern and western Europeans, and for region is Northeast and Midwest.

Source: IPUMS, 5% Census sample, 1930.

*** $p < 0.01$ ** $p < 0.05$

states, and first look at birthplace as a predictor of citizenship acquisition. Converting the point estimates in table 3 into predicted probabilities, we find that Mexicans were 56 percent less likely to be naturalized than the reference group of English Canadians and northern and western Europeans, while southern and eastern Europeans were 18 percent less likely to be naturalized than the reference group.⁷

Next, we add measures for resources, skills, and motivations. Table 3 shows that, as expected, these variables are significant predictors of naturalization. For a substantive sense of these effects, we estimate the effect of each variable on the "median" immigrant's probability of naturalizing.⁸ Speaking English made an immigrant 38 percent more likely to be naturalized compared to one who did not; being literate made him 23 percent more likely to be naturalized. Time in the United States, service in World War I, and hailing from a border country have similar substantive effects: the likelihood of naturalization increases with length of residency (24 percent higher for those who had lived in the United States for 30 as opposed to 15 years); WWI veterans were 22 percent more likely to naturalize; and migrants from a border country were 19 percent less likely to naturalize than Europeans, all else equal.⁹ In comparison, changes in the probability of naturalization were more modest for those who owned a home (+6 percent), were married with a spouse present (+7 percent), had children in their household (+3 percent), or were married but whose spouse was absent (-3 percent).

Taken together, resources, skills, and motivations explain half of the gap in the predicted probability of naturalization between southern and eastern Europeans and the reference group, and explain over 30 percent of the gap between Mexicans and the reference group. Once these variables are taken into account, Mexicans are 38 percent less likely to be naturalized than English Canadians and northern or western Europeans. This large citizenship gap contrasts sharply with southern and eastern Europeans, who are only 9 percent less likely to be naturalized than the reference group once individual-level controls are introduced.¹⁰

To assess the effect of color status on naturalization, we replicate model 1 in table 3, but distinguish between Mexicans who were classified as white by Census enumerators and those classified as non-white. Table 4 shows that the statistical results for non-white Mexicans look very similar to those for all Mexicans in the earlier model: non-white Mexicans were 57 percent less likely to be naturalized than the reference group. (This is not surprising, since 96 percent of Mexican immigrants were classified as non-white.) Importantly, we do see a difference with white Mexicans: they are 47 percent less likely to be naturalized. Just as in model 1, table 3, southern and eastern Europeans are only 18 percent less likely to be naturalized.¹¹

Next, we replicate model 2 from table 3 to consider how resources, skills, and motivations affect the results. As in table 3, the addition of these variables explains over 30 percent of the gap in the predicted probability of naturalization for non-white Mexicans (see figure 1); non-white Mexicans are now 39 percent less likely to be naturalized, less than before, but still a very sizeable difference. For white Mexicans, however, controlling for resources, skills, and motivations explains almost 60 percent of the gap in the predicted probability of naturalization

Table 4. Determinants of Naturalization for Foreign-Born Men, 21 Years and over, 1930

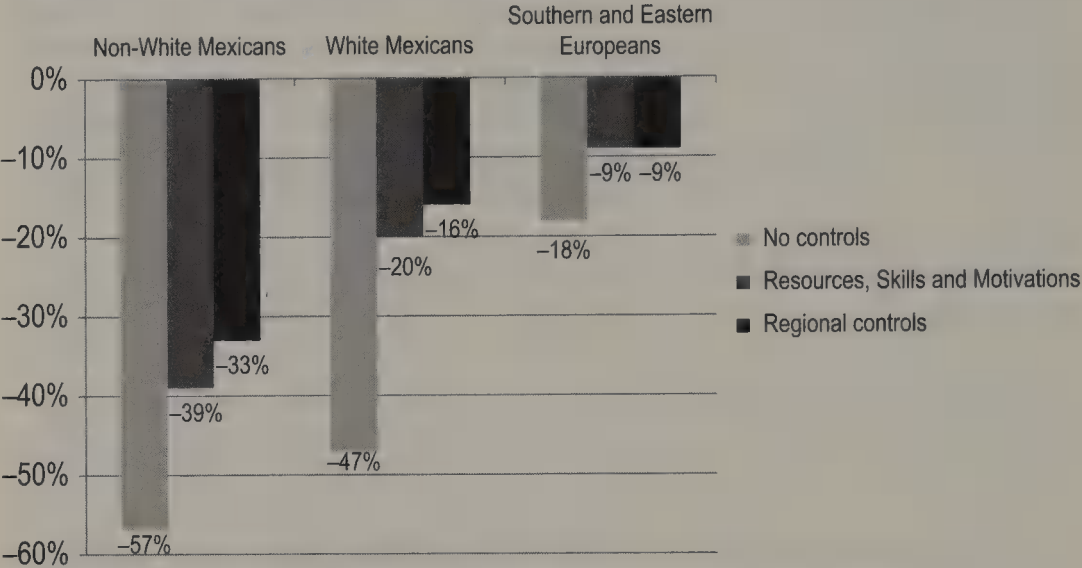
VARIABLES	(1) National origin	(2) Resources, skills, and motivations	(3) Regional context
Non-white Mexican	-3.715*** (0.251)	-1.693*** (0.276)	-1.408*** (0.216)
White Mexican	-2.294*** (0.143)	-0.826*** (0.210)	-0.648*** (0.160)
Southern & eastern European	-0.871*** (0.106)	-0.441*** (0.087)	-0.451*** (0.085)
French Canadian	-0.981*** (0.130)	-0.378*** (0.090)	-0.408*** (0.088)
Years in USA		0.071*** (0.003)	0.071*** (0.003)
Age at migration		-0.009*** (0.001)	-0.009*** (0.001)
Literate		0.961*** (0.070)	0.966*** (0.070)
Speaks English		1.621*** (0.083)	1.620*** (0.081)
SEI score		0.016*** (0.001)	0.016*** (0.000)
Owens home		0.295*** (0.049)	0.299*** (0.046)
Children		0.153*** (0.023)	0.141*** (0.025)
Married, spouse present		0.321*** (0.023)	0.316*** (0.020)
Married, spouse absent		-0.117** (0.051)	-0.126*** (0.045)
Border country		-0.780*** (0.077)	-0.778*** (0.082)
Served in WWI		1.372*** (0.050)	1.380*** (0.051)
South			-0.170 (0.113)
Southwest			-0.388*** (0.138)
Northwest			0.020 (0.156)
Constant	1.295*** (0.085)	-3.801*** (0.096)	-3.775*** (0.109)
Observations	324,863	324,863	324,863

Note: Robust standard errors in brackets. The reference group for origin is English Canadians and northern and western Europeans, and for region is Northeast and Midwest.

Source: IPUMS, 5% Census sample, 1930.

*** $p < 0.01$ ** $p < 0.05$

Figure 1. Gaps in predicted probabilities of being naturalized for white and non-white Mexicans and southern and eastern Europeans*



Note: *Reference group is English Canadians and Northern and Western Europeans
Source: IPUMS, 5% Census Sample, 1930

with the reference group. Compared to the reference group, white Mexicans are 20 percent less likely to be naturalized, and southern and eastern Europeans only 9 percent less likely.¹²

Finally, we look at the role of regional context, the final column of table 4. As predicted, those who lived in the Southwest were 9 percent less likely than immigrants in the Northeast and Midwest to be naturalized; living in the South or Northwest had no significant effect. After taking regional controls into account, non-white Mexicans are now 33 percent less likely to be naturalized than the reference group. In comparison, white Mexicans are now only 16 percent less likely to be naturalized. Regional controls do little, however, to explain the remaining variation in naturalization levels between southern and eastern Europeans and the reference group; the former are still 9 percent less likely to be naturalized than the latter. Overall, the gap between white Mexicans and southern and eastern Europeans has been significantly diminished and, in fact, the difference between the two is no longer statistically significant.

The reduction in the Mexican-European naturalization gap, after introducing regional controls, could suggest that region was especially important for Mexican naturalization, but it could also reflect the fact that southern and eastern European and northern and western European migrants did not experience region differently from each other. Europeans, regardless of national origin, might have experienced region differently from Mexicans. The latter is in fact the case. Based on the interaction between region and national origin, we find that the negative regional context effect of the Southwest applied to European immigrants: *all* European immigrants were significantly less likely to be naturalized if they lived in the Southwest as opposed to the Northeast and Midwest. In other words, the probability that the median European immigrant

would be naturalized was 70 percent if he lived in the Northeast or Midwest, where the vast majority lived, but 61 percent if he lived in the Southwest. For the typical southern and eastern European immigrant, the probability of being naturalized was 64 percent in the Northeast or Midwest but only 50 percent in the Southwest.

In contrast, region of residence was largely insignificant for Mexicans: living in the Southwest, Northeast, or Midwest, where 91 percent of all Mexican migrants resided, made little difference; the median non-white Mexican immigrant's probability of being naturalized was just 12 percent in each of these regions, while the median white Mexican immigrant's probability of being naturalized was 25 percent in each.¹³ The lack of regional variation suggests that it is unlikely that Mexicans could have improved their chances of naturalization simply by moving to the Northeast and Midwest. For Mexicans, the national context of reception, notably the vast majority's non-white color status, mattered most, not regional differences. In contrast, where one lived mattered for European migrants, regardless of national origin. This suggests that part of the gulf in naturalization between non-white Mexicans and Europeans might have closed had more European immigrants lived in the Southwest, which was a less welcoming place for them.

Conclusion

Scholars rightly underscore how racial prerequisites for naturalization and court decisions helped define the boundaries of whiteness and notions of race, deservingness, and membership (Gross 2008; Haney López 1996; Smith 1997). But our research highlights the equally important conclusion that being deemed "white by law" was no guarantee to gaining citizenship through naturalization. In the early 20th century, even though Mexicans were eligible to naturalize, they rarely acquired citizenship in practice. Differences in resources, skills, and motivations—the standard approach to understanding variation in naturalization among Europeans—explain 41 percent of the citizenship gap between Mexican immigrants, on the one hand, and all Europeans and Canadians, on the other. But over half of the variation between these groups remains unexplained.

We argue that sociologists' attention to contexts of reception in the contemporary period also helps us understand naturalization patterns and immigrant integration a century ago. In particular, social conceptions of race or color status and the civic, economic, and political structures operating in specific regions influenced naturalization. The regional concentration of Europeans in the Northeast and Midwest increased their probability of naturalizing, while the effect of color status is clearly visible when we compare the experiences of white and non-white Mexicans. In fact, even after controlling for resources, skills, motivations, and region of residence, the probability that non-white Mexicans would be naturalized was just 33 percent, compared to 51 percent for white Mexicans.

We may well have underestimated the effect of non-white social status on Mexicans' naturalization. Color-based racism may be responsible for some of the

differences in resources, skills, and motivations between non-white Mexican and white Mexican or European immigrants. For example, discrimination in the labor market likely reduced non-white Mexicans' wages, while non-white Mexicans' modest English ability may have stemmed, in part, from high residential, educational, and occupational segregation rooted in racism. Since benign Americanization efforts—efforts that helped immigrants learn English and demonstrate knowledge of the US Constitution for naturalization—targeted southern and eastern Europeans (King 2000), Mexicans probably did not benefit much from free adult night schools, even when programs were available. We consequently believe that our results represent lower-bound estimates of the effect of non-white social status on naturalization.

Although our analysis explains much of the naturalization difference between white Mexicans and southern and eastern Europeans, a small difference remains between these groups (alongside French Canadians) and their northern or western European peers. Discrimination on the basis of religion, nationality, or race may explain some of the remaining gaps. Although southern and eastern Europeans were largely perceived and treated as “white,” they were still seen as racially inferior to other white Europeans, and suffered significant discrimination as a result. Such discrimination may have depressed naturalization among white Mexicans as well as southern and eastern Europeans. Moreover, unlike northern and western Europeans, who were predominately Protestant, most Mexicans (and French Canadians) were Catholic and most southern and eastern Europeans were Catholic or Jewish. The US Census does not collect information on religious identification, so we are unable to evaluate this possibility, but anti-Catholic or anti-Semitic sentiment may have also depressed naturalization.¹⁴ Finally, while discrimination against southern and eastern Europeans was, by all accounts, significant, coming from a border country had a greater dampening effect on naturalization. That is, our model suggests that English Canadians were less likely to naturalize than southern and eastern Europeans, holding all other variables equal.

Our research carries important implications for studies of citizenship and immigrant incorporation. Many earlier studies of European immigrants' incorporation a century ago focused on the urban areas of the Northeast and Midwest, where the majority of these immigrants lived (e.g., Bernard 1936; Lieberman 1963). Building on more recent work that sketches a national picture of historical immigrant integration (e.g., Alba and Nee 2003; Perlmann 2005), we evaluate how attention to regional contexts of reception and non-white social status, separately and together, deepens our knowledge. Our research shows that high levels of citizenship acquisition for European immigrants were regionally embedded. We thus add to a growing body of historical case studies and recent synthetic work that argues that an undifferentiated account of the past is inaccurate (e.g., Benton-Cohen 2009; Fox and Guglielmo 2012). If more Europeans had lived in the Southwest, where municipal reformers tried to repel political machines and where Americanization efforts were more coercive than benign, European immigrants' overall experiences might have been characterized by greater political and legal exclusion. Conversely, being judged non-white in practice, as was the case for the vast majority of

Mexicans, had a significant effect on the political incorporation of Mexican immigrants across the country in the early 20th century, despite their legal classification by the federal government and courts. In addition to our empirical understanding of the past, we concentrate on the consequential acquisition of US citizenship. American citizenship could provide benefits such as WPA jobs during the Depression, access to the ballot box, and protection against deportation (Fox 2012). Low Mexican naturalization might thus tell a broader story about impeded integration, not just in 1930, but beyond. Finally, our analysis highlights the significance of on-the-ground racial boundary formation for political and civic inclusion: being white by law was simply not enough.

Notes

1. In 1868, the Fourteenth Amendment enshrined the right of birthplace citizenship for those born in the United States, irrespective of race. This constitutional protection was upheld by the 1898 Supreme Court decision in *United States v. Wong Kim Ark*. The court found that even if Chinese parents were barred from naturalization on racial grounds, their US-born children were US citizens under the Fourteenth Amendment.
2. We follow the Dillingham Commission definition of northern and western European versus southern and eastern European (US Immigration Commission 1911). In the appendix (table A.1), we provide a detailed table of naturalization rates for Mexicans, Canadians, southern and eastern Europeans, and northern and western Europeans.
3. Explicit exclusions of Asian migrants were first written into law with the 1882 Chinese Exclusion Act, which deemed Chinese migrants ineligible for naturalization on racial grounds.
4. We confine our discussion to the Southwest and the Northeast and Midwest because the vast majority of Mexican (91 percent), northern and western (84 percent), and southern and eastern European (92 percent) immigrants lived in one of these two regions.
5. We also exclude foreign-born individuals whose father was US born (697 cases), since they might have derived citizenship from their father rather than needing to naturalize.
6. Sixty-five percent of Canadians were recorded as "English Canadians" and 30 percent as "French Canadians." The remaining 5 percent were classified in other categories, including simply "Canadian," or by province. Since 95 percent of these "other Canadians" indicated "English" as their mother tongue, we include them with "English Canadians." None of the results are sensitive to this specification.
7. French Canadians were 24 percent less likely to be naturalized.
8. The "median" immigrant in our sample was born in southern or eastern Europe, migrated at the age of 20, had lived in the United States for 25 years, was literate, spoke English, had an SEI of 17, was married with children living with him, did not own a home, did not serve in WWI, was white, and lived in the Northeast or Midwest.
9. As discussed above, we conceptualize the border country effect to capture migrants' beliefs of return and administrative difficulties in naturalizing due to entry via a land border. In trying to model this effect, the coefficient on border country becomes identical to what we would have obtained if we instead had included a dummy variable for English Canadians (see table 4, models (2) and (3)). Doing so would, however, have made it more difficult to disentangle the effect of living in a border country from the origin or race effects for Mexicans and French Canadians.
10. French Canadians are 9 percent less likely to be naturalized.

- 11. French Canadians are 24 percent less likely to be naturalized.
- 12. French Canadians are only 9 percent less likely to be naturalized than the reference group.
- 13. We also found that Mexicans in the South were more likely to be naturalized than Mexicans in other regions. This is consistent with some historical scholarship, which suggests that the small number of Mexicans in the South were more socially accepted (Weise 2008). But since our southern sample includes only 68 Mexican men, we hesitate to make much of this finding.
- 14. Other factors might also play a role, such as sending countries' rules about dual citizenship and the penalties of losing citizenship, including property rights, in homelands that viewed acquisition of US citizenship as a renunciation of one's prior nationality.

Appendix A

Table A1. Citizenship Status of Select Groups of Immigrant Men, 21 Years and over, 1930

	% Naturalized	N
Mexican	9	11,936
Southern and eastern European	60	165,548
<i>Austria</i>	70	9,477
<i>Greece</i>	52	6,067
<i>Czechoslovakia</i>	69	11,686
<i>Italy</i>	58	47,922
<i>Hungary</i>	62	6,388
<i>Poland</i>	58	32,077
<i>Yugoslavia</i>	50	5,942
<i>Lithuania</i>	53	5,363
<i>Russia</i>	70	29,900
Northern and western European	80	122,945
<i>Ireland</i>	85	16,876
<i>England</i>	77	18,299
<i>Sweden</i>	79	15,210
<i>Germany</i>	85	35,405
<i>Scotland</i>	71	7,091
<i>Norway</i>	79	8,854
<i>Denmark</i>	83	5,076
Canadian	65	23,976

Source: IPUMS, 5% Census sample, 1930.

Appendix B

Table B1. Descriptive Statistics, English and French Canadian Immigrant Men, 21 Years and over, 1930

	English Canadians	French Canadians
Naturalized citizen (%)	68	58
Resources, skills, and motivations		
Time in US (yrs)	31	32
Speaks English (%)	99	95
Literate (%)	99	91
SEI score ^a (0–96)	31	21
Age at migration (yrs)	19	17
Own a home (%)	50	42
Married, spouse present (%)	70	71
Married, spouse absent (%)	4	4
Children in household (%)	51	60
From border country (%)	100	100
Served in WWI (%)	7	6
Racism		
Classified as “white” (%)	99	100
Region		
Northeast & Midwest (%)	74	93
South (%)	2	1
Southwest (%)	11	2
Northwest (%)	13	4
Observations	16,366	7,610

^aCalculated for those in the labor force only.

Source: IPUMS, 5% Census sample, 1930.

About the Authors

Cybelle Fox is Assistant Professor of Sociology at the University of California–Berkeley. She studies race, immigration, and the American welfare state. Her most recent book, *Three Worlds of Relief* (Princeton University Press, 2012), compares the incorporation of blacks, Mexicans, and European immigrants in the American welfare system from the Progressive Era to the New Deal.

Irene Bloemraad is Associate Professor of Sociology and the Thomas Garden Barnes Chair of Canadian Studies at the University of California–Berkeley. She studies the intersection of immigration and politics, with a focus on citizenship, multiculturalism, and mobilization in North America and Western Europe. Publications include *Rallying for Immigrant Rights* (2011, with K. Voss), *Civic Hopes and Political Realities* (2008, with S. K. Ramakrishnan), and *Becoming a Citizen* (2006).

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Hispanics at the Starting Line: Poverty among Newborn Infants in Established Gateways and New Destinations

Daniel T. Lichter, *Cornell University*

Scott R. Sanders, *Brigham Young University*

Kenneth M. Johnson, *University of New Hampshire*

High rates of Hispanic fertility raise an important question: Do Hispanic newborn babies start life's race behind the starting line, poor and disadvantaged? To address this question, we link the newborn infants identified with the new fertility question in the 2006–2010 *American Community Survey* (ACS) to the poverty status of mothers. Our results document the disproportionately large share (40 percent) of Hispanic babies who are born into poverty. The prospect of poverty is especially high in new Hispanic destinations, especially those in rural areas. For Hispanic newborn babies, poverty cannot be reduced to supply-side explanations that emphasize maladaptive behavioral decision-making of parents, that is, nonmarital or teen childbearing, low educational attainment, acquisition of English language skills, or other dimensions of human capital. Hispanics in new destinations often start well behind the starting line—in poverty and with limited opportunities for upward mobility and an inadequate welfare safety net. The recent concentration of Hispanic poverty in new immigrant destinations portends continuing intergenerational inequality as today's newborn infants make their way to productive adult roles.

Introduction

The recent spatial dispersion of America's Hispanics from immigrant gateways in the Southwest to new destinations in the Southeast, Pacific Northwest, and agricultural heartland has been both unprecedented and unexpected (Marrow 2013; Singer 2009). In rural America, a burgeoning Hispanic population often provides

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a demographic lifeline to dying small towns (Carr, Lichter, and Kefalas 2012). Hispanics accounted for 56 percent of nonmetro population growth over the early 2000s, but represented only 7 percent of the nonmetro population in 2010 (Lichter 2012). Not surprisingly, a rapidly expanding literature on new Hispanic destinations has focused heavily on immigrant incorporation and community impacts, including poverty and inequality (Parrado and Kandel 2010), local politics and race relations (Okamoto and Ebert 2010), crime and social disorganization (Crowley and Lichter 2009), residential segregation (Hall 2013), and schooling (Dondero and Muller 2012), among other topics. Growing racial and ethnic diversity—and its many social, economic, and political implications—are being played out unevenly across America’s cities, suburbs, and small towns.

The current emphasis on immigrant incorporation reflects the commonplace assumption that population growth in new destinations is driven largely by in-migration of the Hispanic foreign-born population. The fact that geographic and social mobility usually go hand-in-hand implies that spatial assimilation with the majority population is a key dimension of Hispanic economic incorporation. Yet, a large but unappreciated share of the recent growth of Hispanics nationally is the result of fertility, not immigration (Johnson and Lichter 2008). Fertility represents a significant second-order effect of past and current immigration. Fertility has fueled rapid population growth, yet we have only a limited understanding of the disadvantaged circumstances of Hispanic newborn infants as they proceed from childhood into productive adult roles. Processes of assimilation—including spatial assimilation—cannot be fully understood in isolation from currently high and uneven rates of fertility and poverty among Hispanics in new receiving areas. Indeed, diversity and economic incorporation are occurring from the “bottom up”—with infants and children leading the way.

In this paper, we examine the comparative economic circumstances of Hispanics in new destinations but, unlike previous studies, we place the emphasis squarely on newly born infants. That is, do Hispanic newborn babies start life’s race behind the starting line, poor and materially disadvantaged? The answer has implications for later educational achievement, positive developmental trajectories, and transitions to productive adult roles (Clotfelter, Ladd, and Vigdor 2012; Duncan, Ziol-Guest, and Kalil 2010). Nearly one-quarter of all US births today are to Latinas (Martin et al. 2013); today’s Hispanic children and youth will play an important—and growing—role in America’s economic future and in the cities and communities in which they now live. In the absence of upward socioeconomic mobility, childhood poverty may breed poverty as adults (Borjas 2011), a statistical fact that takes on special significance during the current period of apparently declining intergenerational mobility and the emergence of a more rigid American class structure (McCall and Percheski 2010).

In this paper, we use the new fertility question in the 2006–2010 American Community Survey (ACS) to link, for the first time, the records of newborn infants to the poverty status of their mothers. We are unaware of any studies that provide up-to-date national estimates of shares of children born into poverty. We have three specific objectives—each framed conceptually by a model of spatial assimilation. First, as an empirical baseline, we document ethnoracial variation in patterns of poverty among America’s newborn infants, distinguishing the disadvantaged circumstances of Hispanic newborns from other ethnoracial

groups. Second, we highlight “at risk” infants, focusing on variation in the incidence of poverty in new and established Hispanic destinations. We show that the geographical context of reception matters for Hispanics. Third, we highlight the demographic and sociocultural origins (i.e., risk factors) of high rates of poverty among Hispanic newborn infants, including nativity status, family background, and human capital of parents. Our paper contributes to a growing literature on assimilation in new destinations by focusing specifically on impoverished newborn infants in cities and communities that are often unprepared for rapid population growth and increasing racial and ethnic diversity.

For children, the period in utero and during early infancy is especially critical for brain development and later cognitive, emotional, and physical outcomes (Knudsen et al. 2006). Brooks-Gunn and Duncan (1997), for example, showed that early childhood economic conditions rather than current poverty had the largest effects on adolescent cognitive development and achievement. Poor infants face clear developmental disadvantages that persist into adulthood. For the first time in US history, the US Census Bureau reported that the majority of America’s babies in 2011 were born to historically disadvantaged racial and ethnic minority women (i.e., groups other than non-Hispanic whites) (Cauchon and Overberg 2012). As we show here, the growth of the Hispanic population in new destinations, especially in rural areas, is spurred by high fertility, a situation that raises important questions about Hispanic integration and incipient patterns of economic and spatial inequality.

Background: Spatial Assimilation among Hispanics

Fertility and Poverty

Traditional theoretical models of spatial assimilation—those originating from the Chicago School—have been upended by the dispersion of immigrant minority populations to new destinations. The conventional view is that economic, cultural, and political incorporation in established gateways or immigrant enclaves provides a platform for immigrant geographic mobility—to attain better housing elsewhere, and to live in nicer neighborhoods in middle-class communities with more opportunities for their children (e.g., suburban communities). Indeed, to get ahead in life often requires moving elsewhere. Today, however, the unprecedented geographic spread of historically disadvantaged Hispanic populations from established gateways to new destinations represents a singular example of how economic globalism has fundamentally reshaped patterns of transnational labor mobility and a new ghettoization of rural immigrant groups (Crowley and Ebert 2014; Massey 2008).

In the United States, the geographic dispersion of Hispanics has been exacerbated by anti-immigrant legislation in traditional gateway states, the militarization of the Mexican-US border (which has affected points of entry), the restructuring of the meatpacking industry, and America’s continuing demand for low-wage, low-skill workers in the service industry and agriculture (e.g., Kandel and Parrado 2005; Massey and Sanchez 2010). The growth of Hispanics in new destinations has raised the specter of concentrated poverty and spatial inequality (e.g., the rise in majority-minority places), rural ghettos, and Hispanic boomtowns

(Burton, Garrett-Peters, and Eason 2011; Lichter, Parisi, and Taquino 2012b). Iconic images of the disadvantaged migrant farmworker (especially during the Bracero period) have also been replaced—at least in part—by a “settling in” of Hispanic workers in local communities (Marrow 2011).¹ Immigrants are increasingly putting down roots, buying homes, and getting married (or bringing their spouses from elsewhere) (Farmer and Moon 2009). They also are having children and raising families (Lichter, Johnson, Turner, and Churilla 2012a).

Among immigrant populations, low and declining fertility is sometimes regarded as a proxy indicator of economic incorporation and assimilation (Bean, Swicegood, and Berg 2000; Parrado and Morgan 2008). Low fertility both reflects and reinforces upward social mobility, which is revealed in the short- and long-term economic trajectories of children (Stevens 1981). Conversely, current patterns of Hispanic fertility have placed upward demographic pressure on poverty rates in those communities and regions where Hispanic workers and their families have relocated. Growing poverty presumably reflects the fact that: (1) childbearing among Hispanics is higher on average than among non-Hispanics (Tienda and Mitchell 2006); (2) poverty rates are higher on average among fast-growing Hispanic populations (e.g., Mexicans) than other groups (Orrenius and Zavodny 2013); and (3) Hispanic childbearing is highest among the poorest, least educated, and more disadvantaged (e.g., non-citizens, non-English speakers, etc.) (Lichter et al. 2012a). We consider the substantive implications of each point in turn below.

High rates of Hispanic fertility are sometimes couched in cultural rather than economic terms, although “familism” as a driver of Hispanic fertility remains a debatable and contentious issue (Hartnett and Parrado 2012). US Hispanic births peaked in 2007 at nearly 1.1 million, and 2010 produced the lowest annual number of Hispanic births since 2003. With the 2000s Great Recession, traditional patterns of family formation—even among Hispanics—have been disrupted, if measured by declining rates of marriage and fertility. Recent estimates from the National Center of Health Statistics nevertheless continue to show much higher fertility rates among Hispanics than whites or other US minority populations (Martin et al. 2013). For example, the General Fertility Rate (GFR) among Hispanics (i.e., the number of births per 1000 woman of reproductive age) was 80.2 in 2010, well above the overall US rate of 64.1 (Martin et al. 2013). For non-Hispanic whites, the GFR was 58.7. The current total fertility rate among Hispanics also is well above replacement levels (2.34), and 30 percent higher than the rate of Whites. The implication is clear: high Hispanic fertility is driving America’s new diversity, starting with newborns.

High rates of Hispanic fertility also have contributed to rapid shifts in the racial and ethnic profile of US poverty. The 2014 March Current Population Survey indicates that 14.7 million US children were poor in 2013, of which 10.6 or over 70 percent were racial or ethnic minorities, that is, children who were identified by membership in groups other than non-Hispanic white (author’s calculations; US Census Bureau 2014). Today, over 5.4 million US Latino children live in poverty, a number that exceeds the number of poor white children and every other racial or ethnic minority group (US Census Bureau 2014). Latino children comprise 23.1 percent of America’s children, but 37.3 percent of its poor

children. Poverty rates among Hispanics are high by national standards—26.6 percent in 2010 (DeNavas-Walt, Proctor, and Smith 2011)—but they are especially high (34.9 percent) among Hispanic children (Lopez and Valasco 2011).² In the absence of upward socioeconomic mobility or appropriate policy interventions (e.g., early childhood education or a strong safety net), Hispanic immigration—and the second-order effects of above-replacement fertility—will alter the demographic profile of US poverty as today’s minority children proceed toward adulthood.

Finally, previous demographic studies of Hispanic childbearing show clear socioeconomic gradients in the tempo and quantum of fertility; that is, early and cumulative fertility declines with more education, higher family incomes, and lower poverty rates (Carter 2000; Stevens 1981). Low-SES groups contribute disproportionately to the number of Hispanic newborns (DeLeone, Lichter, and Strawderman 2009). The implication—one often drawn without direct evidence—is that the recent uptick in the number of poor children reflects high fertility rates among the most disadvantaged segments of America’s diversifying population (i.e., low-educated and poor Hispanics).³ More importantly, the growth of poor infants will occur disproportionately in places where Hispanics are concentrated—in new destinations and established gateways. Fertility and poverty are inextricably linked, and expressed in disproportionate shares of Hispanic infants who are born into poverty.

Born Poor in Hispanic Receiving Areas

From a theoretical standpoint, high poverty rates among Hispanic newborn infants are a product of the disadvantaged circumstances of Hispanic mothers—impoverished family backgrounds, early family formation, and chronic deficits in human and social capital. Job discrimination (and its correlates of worker exploitation and wage theft) may also play a role in denying access to jobs that pay a living wage. For example, Hispanic mothers of newborn infants are more likely to be high school dropouts, unemployed, or not in the labor force, and presumably they are less skilled and experienced if employed (Crowley, Lichter, and Qian 2006). Traditional gender roles may magnify the lack of opportunity among new mothers.

It also is significant that Latinas on average begin childbearing at younger ages than other minority populations (Lopez and Valasco 2011; Martin et al. 2013), and Hispanic families are typically larger in size, which means that more family income is required to meet or exceed the official poverty threshold. High fertility and family size dilute family resources and elevate the prospect of poverty. For young mothers, in particular, wages are expected to be lower and poverty rates are likely to be higher than for older mothers. Among young mothers, including teen mothers, marriage rates also are lower and nonmarital fertility rates are higher (DeLeone et al. 2009), which diminishes the likelihood of child support or other financial assistance from fathers. One-half of all Hispanic births today are to unmarried women (Martin et al. 2013), and poverty rates of Hispanic children are especially high when children live alone with their unmarried mothers (nearly 60 percent; see Lopez and Valasco [2011]). Hispanic mothers, especially those who have recently arrived in the United States as foreign-born immigrants, may also be ineligible for work-based cash

assistance programs, such as TANF, or other government programs (SNAP) that benefit children (Borjas 2011).

Hispanic newborn infants clearly are “at risk” of poverty due in part to the disadvantaged circumstances of their mothers, current family living arrangements, and limited access to America’s social safety net. But poverty is not simply a reflection of selection of mostly disadvantaged Hispanic families (or mothers) into immigrant settlement areas. Poverty also reflects demand-side characteristics (Crowley and Ebert 2014; Hyde, Pais, and Wallace 2015). Our working hypothesis is that poverty is overrepresented among Hispanic newborn infants in immigrant receiving communities, independent of supply-side factors. Although some new destinations have become safe havens from the anti-immigrant political climate increasingly found in some established destinations (e.g., in Arizona and California) (Massey and Sánchez 2010), geographic isolation or spatial concentration elsewhere arguably is no economic panacea. On the demand side, the jobs available to Hispanics in agriculture, nondurable manufacturing (e.g., meat and food processing), and services (e.g., landscaping) often do not pay a living wage (Curtis, Voss, and Long 2012). New Hispanic destinations also lack the institutional support services (e.g., culturally sensitive healthcare) that make economic integration possible, at least as expressed in jobs that pay a decent wage. And, unlike established gateways, new destinations may lack mature or dense social networks that provide informal support and economic assistance to newcomers (Bachmeier 2013).

In Hispanic boomtowns, the extent and depth of poverty also is exacerbated if economically disadvantaged Hispanic families have higher than average rates of fertility. In many new destinations, especially isolated rural communities with aging white populations and chronic net outmigration, the majority of births are to Hispanic mothers Lichter 2012. Recent estimates indicate comparatively high rates of Hispanic childbearing in new immigrant destinations. Using data from the 2005–2009 American Community Survey, Lichter et al. (2012a) reported a GFR of 92 among rural Hispanics compared with a GFR of 76 among their metro counterparts. The demographic implications of high Hispanic fertility are magnified in economically depressed places, where aging-in-place and chronic outmigration have depleted the native-born white population of childbearing age (Johnson and Lichter 2008). Hispanics increased from less than 3 percent of the nonmetro population in 1990 to 7.4 percent in 2010 (Economic Research Service 2013; US Census Bureau 1993).

Poor Hispanic populations also have become increasingly concentrated in poor neighborhoods, communities, or regions, which exacerbates the problems of poor families (e.g., lack of job opportunities or educational opportunities). A recent USDA study, for example, showed that nearly 40 percent of rural Hispanics lived in high-poverty counties, defined as having poverty rates of 20 percent or more (Farrigan and Parker 2012); this figure compared with about 18 percent of poor Hispanics living in poor metro areas. The share of all Hispanics living in high-poverty counties increased during the early 2000s by nearly 10 percentage points. The number of high-poverty areas also increased after declining in the 1990s (cf. Lichter et al. 2012b), as did the number of poor suburban communities located outside principal cities (Kneebone, Nadeau, and Berube 2011). Farrigan and Parker (2012) identified 193 new high-poverty nonmetro

counties and 55 new high-poverty metro counties that emerged in the 2000s. For people living in low-income communities, poverty is chronic, reinforced by high unemployment and too few jobs. Such areas also are home to underfunded schools and limited opportunities for upward mobility as children grow into adolescence and young adulthood (Dondero and Muller 2012).

Poor minority communities often lack access to reproductive health services, including low-cost contraception, and nearby abortion providers in many rural communities and states (e.g., the Dakotas, Mississippi, Idaho, etc.) are limited (National Campaign to Prevent Teen and Unintended Pregnancy 2013). Under the circumstances—and regardless of the economic circumstances of minority families—nonmarital childbearing is often higher than average in economically depressed neighborhoods and communities (South and Crowder 2010), which reinforces poverty from generation to generation and further concentrates minority poverty in poor communities. These problems are expected to be most acute in rural areas with large recent influxes of low-wage, low-skill Hispanic workers. The implication is clear: fertility and assimilation, including spatial assimilation, are inexorably linked.

Current Study

For many Hispanic areas of new settlement, an older, largely nonpoor white population will increasingly be replaced over the foreseeable future by today's younger, disproportionately poor minority population (Lichter 2013). This racial and ethnic transformation will occur first and most rapidly in today's established and new Hispanic boomtowns, which are rapidly diversifying from the "bottom up." As we have argued here, current patterns of childbearing may exacerbate poverty and spatial inequality, and slow the process of economic incorporation, especially if births to poor Hispanic populations are concentrated in poor and neglected places.

We hypothesize that (1) Hispanic newborn infants will experience very high rates of poverty, an empirical fact that we document here for the first time; (2) recent poverty rates are overrepresented among Hispanic newborn infants, especially in the new Hispanic destinations located in rural areas; and (3) significant shares of the Hispanic infant poverty gap—but not all of it—can be attributed to the social, economic, and spatial disadvantages faced by Hispanic mothers and their families. We also assume here that infant and child development occurs in context. Today's immigrant settlement patterns suggest that America's newest generation of poor Hispanic children faces major obstacles to success—in local school, the workforce, and family life—which ultimately threaten long-term social and economic incorporation and social integration into mainstream society.

Methods

American Community Survey: Fertility and Poverty Data

Our goal, quite literally, is to identify only those infants who were born during a period when their mothers were officially defined by the Census Bureau as poor (i.e., they were "born poor"). Economic information about newborn babies (or their mothers or families) is unavailable from the birth registration system of

the National Center for Health Statistics (NCHS). Moreover, although it is sometimes possible to link fertility (and newborns) to the economic circumstances of their mothers in periodic nationally representative surveys (e.g., National Survey of Family Growth), such efforts typically face serious sample size limitations because fertility is a relatively rare event annually. Another alternative approach, which we ultimately discarded, would be to identify poverty among the 0–1 population using the decennial Census (such as the Public Use Microdata files), but the problem here is that the long form, which included questions on family income used to measure poverty, was eliminated in 2010. And, while the annual March Supplement of the Current Population Survey (CPS) includes the 0–1 population and information on family poverty, the sample sizes are insufficient for our purposes. The time referents for measuring recent fertility and infant poverty in the CPC also are mismatched.

Fortunately, the release of the annual American Community Survey (ACS) now makes it possible for the first time to identify infants born into impoverished families. The sample is of sufficient size to facilitate analyses of rural as well as urban areas and minority populations, including Hispanics. For our purposes, we use annual data from the 2006 through 2010 ACS microdata files to identify newborns and their mothers. First, we identify infants aged 1 year or younger at the time of the survey. Second, we link infants with parent and household information (including poverty status) by merging their files with the mother and household files. Third, to ensure that infants are properly linked to their biological mothers, we use the ACS fertility question (i.e., “Has this person given birth to any children in the past 12 months?”) along with ACS individual and household ID variables.

The time referent of the fertility question (i.e., childbearing in past 12 months) is identical to the time referent for the family income question (i.e., family income in the past 12 months), which is used by the Census Bureau to estimate poverty. By linking infant data to mother and household data, it is possible to estimate the number and share born into poverty. Specifically, infants are identified as poor at birth if they lived in families with incomes below the official poverty income thresholds, as defined by the US Office of Management and Budget. These poverty income thresholds take into account the incomes of all family members, and provide equivalent family incomes for different-sized families (to take into account economies of scale). Whether a newborn is defined as poor depends on total family income, family size, number of children in the family, and age of the householder.

Measurement

In this paper, we first identify all Hispanic newborns from the Hispanic question of the ACS (i.e., Hispanic origin). Hispanics can be of any race. All other newborns are classified as non-Hispanic, that is, non-Hispanic white, non-Hispanic black, and so forth. To identify racial and ethnic variation in poverty, we consider a variety of demographic characteristics, including immigration status and the marital status of the new mother (see Appendix table A1). We identify “at risk” infants whose mothers were teenagers when they began childbearing or were

unmarried at the time of birth. We also consider the number of siblings in the household (at the time of the survey), as well as the number of related adults (who can provide family income as primary and secondary workers). Poverty rates typically increase—quite mechanically—with family size because more family income is required to meet or exceed the higher poverty income thresholds. Two dummy variables are included to differentiate foreign-born mothers who are established immigrants (immigrated to the United States before 2000) from those foreign-born mothers who immigrated to the United States in 2000 or more recently. Previous research shows that economic incorporation increases with duration in the United States (Van Hook, Brown, and Kwenda 2004). Migrant status (whether foreign born or native born) is determined by moves to a different Public Use Microdata Area (PUMA) during the past 12 months (US Census Bureau 2013). We expect recent domestic or international migration to increase the likelihood of poverty due to the potential lack of a support network among transient populations.

We also consider several supply-side factors commonly associated with maternal employment and human capital (Appendix table A1). Specifically, dummy variables indicate whether the mothers of newborn infants speak no or poor English, whether mothers have a high school education or less, or alternatively have some college (with college graduate serving as the reference category), and whether mothers are currently employed. We expect that higher levels of human capital and maternal employment will decrease the likelihood of an infant being born into poverty.

Previous research has used many different levels of geography to define new destinations, including regions, states, counties, and places. The ACS microdata lacks county identifiers, so we identify new destinations here by state of residence, following the practice of other recent studies. We define Hispanic gateways or established destinations as comprising 10 states that, in 1990, accounted for roughly 90 percent of all US Hispanics (US Census Bureau 1993). Established gateways include Arizona, California, Colorado, Florida, Illinois, Massachusetts, New York, New Jersey, New Mexico, and Texas. As expected, these gateway states accounted for a large percentage of all recent Hispanic births (i.e., 76.3 percent). Since 1990, the geographic spread of the nation's Hispanics has accelerated (Johnson and Lichter 2008; Massey 2008). For our purposes, a new Hispanic destination state is one that experienced a 250 percent or more increase in the size of its Hispanic population between 1990 and 2010. Twenty-one states meet this criterion: Alabama, Arkansas, Delaware, Georgia, Indiana, Iowa, Kentucky, Maryland, Minnesota, Mississippi, Nebraska, Nevada, North Carolina, Oklahoma, Oregon, South Carolina, South Dakota, Tennessee, Virginia, Washington, and Wisconsin. These states resonate with our understanding of the geographical location of new destinations, which dominate today in the Southeast (e.g., North Carolina), Mississippi Delta region (Arkansas and Mississippi), agricultural heartland (Iowa and Minnesota), and Pacific Northwest (Oregon and Washington). The residual category of 20 states (including the District of Columbia) also represents slower-growing new destinations (with comparatively small Hispanic populations) outside traditional gateway states (i.e., those with large percentages of Hispanics in 1990).⁴

Analytical Framework

We identify comparative fertility and poverty rates among Hispanic newborn infants in new and established destinations. Logistic regression is appropriate for our purposes because the probability of an infant being born into a family that is either below or above the poverty line is a binary outcome. We adjust standard errors for clustering of births within states and for design effects, as well as conduct additional robustness checks using multilevel modeling techniques.⁵ Adjusting for the design effects of the ACS helps ensure that the results are consistent with the population estimates from the Population Estimations Program of the Census Bureau (US Census Bureau 2009). The relatively unrestrictive assumptions of logistic models are also appropriate when analyzing non-normally distributed variables often associated with poverty. In our analysis, the binary logistic model is expressed as

$$z = \log(p(\gamma_2)/(1 - p(\gamma_1))) = \alpha_0 + \sum \beta_k \chi_{ik},$$

where α_0 is a constant and β_k is the effect of a unit change in independent variable χ_{ik} on z , the log odds of the dichotomous outcome variable (Hosmer and Lemeshow 2004). We report β_k in terms of odds ratios: values greater than 1 indicate that the newborn in family k was more likely to be born into an impoverished family (outcome γ_2) than those in the reference group; values less than 1 indicate that newborns in family k were less likely to be born poor.

Findings

Hispanic Fertility in New Destinations

The results in table 1 reveal a national GFR of 67.7 for 2006–2010 (column 1, line 1). The conventional view of rural America as a repository of traditional family values, at least as measured by fertility, is not evident in the observed GFRs reported in table 1. There are only very modest differences between fertility levels in metro and nonmetro areas.

Yet, Hispanic fertility rates are roughly 20 percent higher than non-Hispanic fertility (77.3 versus 64.2).⁶ Hispanic/non-Hispanic differences in fertility also are larger in nonmetro (88.7 versus 66.6) than metro areas (76.9 versus 63.6). High rates of Hispanic fertility in nonmetro areas are driven largely (but not entirely) by the high fertility of Mexican-origin Hispanics, who tend to be the least educated and skilled, and who typically have poverty rates well in excess of the native-born white population. With a GFR of 92.1 per 1,000 women, the fertility of rural Mexican-origin Hispanics exceeds rates of other historically disadvantaged minority populations, including blacks (71.9) and American Indians (87.1).

The results in table 1 suggest no difference in the fertility rates for those in new destinations, gateways, and other destinations. Spatial differentials in fertility, however, are evident when comparing Hispanics and non-Hispanics. The GFR was 88.9 among Mexican-origin Hispanics in gateway states, but 25 percent higher

Table 1. General Fertility Rate by Race/Ethnicity and Metropolitan Status

	Total	Metro	Nonmetro	Gateway	New destination	Other
Total	67.7	67.5	68.9	67.6	68.5	65.8
Hispanic	77.3	76.9	88.7	79.6	99.0	87.2
Mexican	83.2	82	92.1	88.9	111.1	88.8
Other	76.4	70.5	88.4	70.8	90.1	81.8
Non-Hispanic	64.2	63.6	66.6	62.0	66.2	65.7
White	65.8	64.9	67.1	60.2	64.5	64.1
Black	69.9	69.6	71.9	66.6	71.1	72.7
Asian	70.0	70.0	71.8	67.4	76.2	74.1
American Indian	81.2	76.5	87.1	74.1	82.9	85.2

Source: American Community Survey, 2006–2010

in new destinations (GFR = 111.1). High rates of childbearing among Hispanics, especially Mexican-origin Hispanics, clearly are giving demographic impetus to new diversity—from the bottom up. And this is occurring most rapidly in non-metropolitan areas and new destinations.

Differentials in Poverty among Hispanic Newborns

Our estimates from the 2006–2010 ACS show that 23 percent of America’s infants are born into poverty (table 2). But there are large racial and ethnic differences around this national average. Over one-third (i.e., 34.8 percent) of all Hispanic infants today are born into poor families, a significant figure if considered in tandem with high Hispanic fertility (table 1). Moreover, Hispanic poverty rates are exceptionally high among rural newborn infants (40 percent) and those born in new destinations (41.2 percent). Incorporation clearly is highly segmented geographically. In these Hispanic immigrant-receiving areas, poverty is most pronounced among Mexican-origin infants. Rural poverty rates remain lower among Hispanics than other historically disadvantaged minorities, including rural blacks (55.2 percent) and American Indians (46.6 percent).

The successful integration into American society among today’s newborn infants is likely to be experienced unequally across population subgroups. For example, as shown in table 3 (column 3, line 1), an exceptionally large percentage—70 percent—of all Hispanic infants today are born to mothers with a high school degree or less, and the poverty rate among the infants of these mothers is 42.9 percent.⁷ The low educational levels of Latinas place their newborn infants “at risk” of poverty and other forms of deprivation. Only 12 percent of all Hispanic newborn infants have college-educated mothers, compared with a national figure of 32 percent (see Appendix table A1). Economic incorporation is also highly segmented by place of residence. Indeed, the disadvantaged circumstances of newborn Hispanic infants, if measured by mothers’ education, are most

Table 2. Infant Poverty Rate by Race/Ethnicity and Metropolitan Status

	Total	Metro	Nonmetro	Gateway	New destination	Other
Total	23.0	21.6	29.0	23.0	24.1	21.4
Hispanic	34.8	33.2	40.0	33.7	41.2	35.7
Mexican	37.8	36.2	41.6	35.5	43.1	40.9
Other	30.3	30.1	33.6	29.1	38.8	27.5
Non-Hispanic	19.9	17.8	27.8	17.5	22.8	19.5
White	16.1	13.5	24.3	22.1	20.3	18.7
Black	40.9	38.9	55.2	37.5	42.3	37.6
Asian	11.9	9.6	21.1	10.9	12.5	10.0
American Indian	42.2	36.9	46.6	41.1	44.1	38.5

Source: American Community Survey, 2006–2010

apparent in rural areas, where 76.1 percent have mothers with a high school education or less. Among whites, only 28.7 percent of newborn infants have mothers with a high school degree or less (data not shown).

The life course trajectories of newborn infants also are compromised by other familial and environmental conditions that place them “at risk.” For example, larger shares of rural than urban Hispanic infants are born to teenage mothers (11.7 versus 9.4 percent). About one in four Hispanic newborn infants have mothers who began childbearing as teenagers. The share of Hispanics born to teen mothers was especially high among those who were “born poor” (34.4 percent; data not shown), a figure roughly twice as large as the national figure (17.2 percent) (appendix table A1). And, perhaps most significantly, over 50 percent of all Hispanic infants born to single mothers (i.e., never married or previously married) were poor (i.e., 53.8 percent). This figure is striking, considering that roughly 40 percent of all Hispanic infants are born to single mothers (table 3). Out-of-wedlock childbearing is strongly associated with newborn infant poverty.

Hispanic newborn infants also are likely to have larger numbers of siblings than their non-Hispanic counterparts. Just over 27 percent of Hispanic newborn infants have two or more siblings in the household (table 3). This is higher than non-Hispanics newborns, where only 21.4 percent have two or more siblings (data not shown). In general, poverty rates are highest among families with the most siblings (e.g., over 45 percent among rural Hispanics). Hispanic infants, especially in rural areas, also are much more likely to live in households with additional adult family members, a fact suggesting greater availability of secondary workers and potential adult caretakers. Indeed, over one-half of all Hispanic newborn infants in rural areas were living in households with three or more related adults, compared with only about 20 percent of their newborn counterparts in urban areas. Such results suggest that “doubling up” provides a hedge against poverty in rural areas.

Table 3 also shows that nearly one-half (i.e., 46.9 percent) of all rural Hispanic newborn infants have foreign-born mothers; these newborn infants had

**Table 3. Parental Characteristics of Hispanic Newborn Infants and Percentage Poor
(% Hispanic newborns in poverty in parentheses)**

	Metro	Nonmetro	Total
Education of mother			
High school or less	69.4 (42.5)	76.1 (46.1)	70.0 (42.9)
Some college	17.9 (20.6)	16.8 (25.4)	17.8 (21.0)
College+	12.8 (8.7)	7.2 (8.9)	12.2 (8.7)
Age of mother			
Less than 20	9.4 (46.9)	11.7 (50.8)	9.6 (46.9)
20–24	23.3 (40.2)	28.2 (44.1)	23.7 (40.2)
25+	67.4 (30.3)	60.1 (35.9)	66.7 (30.4)
Marital status			
Single/never married	39.8 (53.3)	40.2 (58.9)	39.8 (53.8)
Married	60.2 (21.6)	59.9 (27.2)	60.1 (22.1)
Nativity of mother			
Native born	47.8 (30.8)	53.2 (36.6)	48.3 (31.4)
Immigrant	52.2 (37.3)	46.9 (43.8)	51.7 (37.9)
First teen birth			
First birth as a teen	25.8 (44.9)	31.0 (49.8)	26.3 (45.5)
First birth as adult	74.2 (30.5)	69.0 (35.6)	73.8 (30.9)
English ability			
No/poor English	12.2 (52.1)	13.3 (55.1)	12.3 (52.4)
Good/excellent English	87.8 (31.7)	86.8 (37.7)	87.7 (32.3)
Employed	42.2 (19.1)	39.3 (24.7)	41.9 (19.6)

(Continued)

Table 3. continued

	Metro	Nonmetro	Total
Unemployed	57.8 (45.2)	60.7 (49.9)	58.1 (45.7)
Number of siblings			
0	47.7 (33.9)	46.5 (40.0)	47.6 (34.5)
1	25.2 (26.3)	25.1 (34.1)	25.2 (27.1)
2+	27.13 (42.0)	28.4 (45.1)	27.3 (42.3)
Number of adults in household			
1–2	79.2 (39.5)	48.7 (45.1)	76.3 (40.1)
3–4	13.6 (20.7)	35.6 (24.7)	15.8 (29.6)
5+	7.1 (23.5)	15.7 (29.8)	7.9 (24.0)
Migrant household			
Migrant	24.6 (44.2)	25.5 (49.2)	24.6 (44.6)
Non-migrant	75.5 (30.9)	74.5 (36.8)	75.4 (31.5)
Geography			
Traditional gateway	79.2 (33.6)	48.7 (39.1)	76.3 (33.9)
New destination	13.6 (36.5)	35.6 (44.8)	15.8 (38.8)
Other destination	7.1 (37.7)	15.7 (36.4)	7.9 (37.4)
Sample size	33,693	3,449	37,142

Source: American Community Survey Five-Year Sample, 2006–2010

a poverty rate of 43.8 percent. The share of newborns to foreign-born Hispanic parents is very similar in rural (47 percent) and urban (52 percent) areas. And a disproportionately large share (about 12 percent) of Hispanic newborns have parents who speak no English or poor English. This compares with only 2.7 percent of all the parents. Not surprisingly, poverty rates are exceptionally high for the infants of foreign-born parents (37.9 percent) and those who speak English poorly (52.4 percent).

The conventional view of spatial assimilation is that metro gateways provide an initial point of entry for Hispanic immigrant populations. Over time and across generations, upwardly mobile (and culturally assimilated) immigrants and their descendants then spread geographically to find new opportunities. Our results instead suggest both large shares of newborn Hispanics living in new rural destinations and high rates of poverty—higher than in established gateways (table 3). The implication is that young foreign-born (and disadvantaged) Hispanics in the family-building stages may have bypassed urban gateways altogether for rural areas.

Modeling Poverty among Newborn Infants

High rates of poverty among Hispanic newborn infants in new destinations and rural areas undoubtedly reflect both negative selection and other causative factors rooted in local opportunity structures. In this section, logistic regression is used to estimate the sources of poverty among Hispanic newborn infants. Table 4 provides odds ratios for all newborn infants (models 1–3) and for Hispanic newborn infants (models 4–5). Odds ratios identify the relative risks of poverty for different demographic segments of the population of newborn infants. Odds ratios above 1 indicate greater relative odds of poverty at birth relative to the reference group.

Our baseline estimates from model 1 reveal an exceptionally high relative risk of poverty among Hispanic newborns. Specifically, the odds of Hispanic poverty are 2.78 times greater than the odds of poverty experienced by non-Hispanic white newborn infants. The odds are even higher among blacks ($OR = 3.33$), but lower among Native Americans ($OR = 1.97$). For 2006–2010, babies with Asian mothers were least likely to be born into poverty; that is, among Asian newborns, the odds of being poor were 38 percent lower than white newborn babies ($OR = .62$).

These racial and ethnic differences in poverty are not due to differences in rural-urban residence patterns, which are controlled in model 1 (table 4). In fact, the relative risk of poverty among nonmetro newborn infants is 1.80 times greater than for infants born to metro mothers. Racial composition cannot fully explain high rates of poverty in nonmetro areas. And, in some additional analysis (not shown), with the race dummies removed from the regressions, the nonmetro estimate is actually slightly larger ($OR = 1.88$) than in model 1. Because newborn infants in rural areas are disproportionately white, race in this case acts as a suppressor variable, masking the large spatial or rural-urban differential in infant poverty.

Model 2 includes dummy variables that identify newborns living in fast-growing and other new destinations, with established gateway states serving as the reference category. These analyses reveal unexpectedly lower odds of poverty among newborns in fast-growing new destinations ($OR = .94$), but higher odds in slower-growing areas ($OR = 1.13$) that perhaps provide fewer job opportunities or less institutional social support. Any evidence of relative disadvantage (shown in tables 2 and 3) seemingly reflects differences in urban-rural residence and racial composition. Previous research examining the economic effects of new

Table 4. Logistic Regression of Poverty of Newborn Infants, 2006–2010 (models are adjusted for ACS clustering and design effects)

Variables	Model 1	Model 2	Model 3	Hispanics only	
	Odds ratio	Odds ratio	Odds ratio	Model 4	Model 5
Household in nonmetro area	1.80***	1.76***	1.45***	1.16**	1.24**
Hispanic	2.78***	2.92***	1.45***	—	—
Black	3.33***	3.33***	1.54***	—	—
Asian	.62***	.64***	.99	—	—
American Indian	1.97***	1.97***	1.27***	—	—
New Hispanic destination		.94***	.95***	.98*	.99
Other destination		1.13***	1.13***	1.20*	1.26*
First birth as a teenager			1.30***	1.26***	1.26***
Unmarried mother			7.24***	6.25***	6.28***
Number of siblings in the household			1.21***	1.19**	1.19**
Number of adults in the household			.66***	.71***	.70***
Foreign-born mother, immigration before 2000			1.12*	.91	.92
Foreign-born mother, immigration after 2000			1.20**	1.36*	1.35*
Migrant household			1.69***	1.48***	1.48***
Mother with no/poor English			1.57***	1.55***	1.55***
Mother education, high school dropout			2.57***	2.54***	2.54***
Mother education, college graduate			.34***	.47***	.47***
Mother employed			.24***	.25***	.25***
Nonmetro × New Hispanic destination					1.20*
Nonmetro × Other state					1.01
Constant	.05	.04	.78	1.35	1.98
Log likelihood	−91651.484	−91572.201	−63423.163	−17675.498	−17672.169
Sample size	181,427	181,427	181,427	37,142	37,142

* $p < .05$ ** $p < .01$ *** $p < .001$

Hispanic destination counties has reported few negative effects on economic conditions and well-being (Crowley and Lichter 2009). The results from model 2 indicate that the odds of poverty are lowest for newborn infants in new destination states.⁸ But, as we show later (with models that include interaction terms), these baseline findings hide the large socioeconomic disadvantages between new destinations in urban and rural areas.

To be sure, racial and spatial disparities in poverty are due, at least in part, to the overrepresentation of “risk factors” among Hispanic newborn infants and those born in rural areas. This inference is drawn from a model that incorporates several risk factors (e.g., teen mothers, whether the mother was married, etc.) for infant poverty (model 3, table 4). When these variables are added to the model (i.e., model 3), the size of the Hispanic odds ratio is cut in half, declining from 2.92 to 1.45.⁹ Much of the difference—but clearly not all—is located in comparative risk factors of Hispanic families vis-à-vis non-Hispanic families. Indeed, based on our calculations from this model (model 3, table 4), the predicted percent poor for Latino newborns is 27.1 percent compared to 20.0 percent among whites, holding everything else in the model constant.

The size of the effects associated with blacks and American Indians is also greatly reduced in model 3. Model 3 accounts for substantially more variation in the infant poverty rates than did model 2, as indicated by the large reduction in the log-likelihood statistic. Still, even with the increased explanatory power of the full model, the odds of poverty remain substantially higher among the newborn infants of historically disadvantaged minority populations. A comparison of models 2 and 3 also underscores the point that the higher odds of poverty among nonmetro newborn infants (OR = 1.76 in model 2 to 1.45 in model 3) and the lower odds among those living in new destinations (.94 to .95) remain intact when all of the risk factors are included in the models. The drop from 1.76 to 1.45 clearly indicates that rural children have many characteristics (e.g., teen mothers, etc.) that place them at greater risk for poverty than their same-race metro counterparts.

This is especially true of Hispanic newborn infants. Model 4 is limited to Hispanics, with the goal of identifying sources of variation in poverty among Hispanic newborns. As with the national sample, these models provide evidence of higher rates of poverty among newborn Hispanic infants in nonmetro areas (OR = 1.16).¹⁰ The results also show that Hispanic infants face significantly higher rates of poverty if their mothers began childbearing as teenagers (OR = 1.26), were unmarried (OR = 6.25), lived in a migrant household (OR = 1.48; i.e., as a measure of transience), or were foreign born, especially if they arrived in the United States after 2000 (OR = 1.36). Newborn Hispanics also face high rates of poverty if their mothers speak little or no English (OR = 1.57), have less than a high school education (OR = 2.57), or are not currently employed (OR = 1/.25 or 4.00).

To put these poverty estimates in perspective, the newborn infants of an unmarried, recent foreign-born immigrant, teen mother who speaks little or no English have an odds ratio of poverty at birth of over 25 vis-à-vis their otherwise similar newborn counterparts whose mothers were married, native born, age 20 or older, and spoke English well. The newborn children of low-educated, low-skilled Hispanic mothers face an exceptionally high risk of poverty. These odds

ratios among single mothers are especially relevant at a time when roughly one-half of all Hispanic births are to unmarried women (Martin et al. 2013).

In some additional analysis, we also tested the hypothesis that Hispanic newborn infants in new rural destinations have greater relative odds of poverty. Specifically, we estimated a final model (model 5, table 4) that includes interaction effects between nonmetro residence and new destinations. These results reveal a large and statistically significant interaction term ($OR = 1.20$). The pattern of interaction—and the large disadvantage facing Hispanic infants in new rural destinations—is illustrated in figure 1. This figure shows that poverty among Hispanic newborn infants is 1.50 times greater in rural new destinations than in metro established gateways. Or, interpreted differently, any deleterious effects associated with being born in a new Hispanic destination are largely limited to rural areas.¹¹

Finally, because a large share of all poor newborn infants have Hispanic foreign-born mothers (i.e., 37.9 percent), we also estimated models separately by nativity status. The results in table 5 show that newborn Hispanic infants of immigrant mothers face especially high rates of poverty vis-à-vis foreign-born white mothers. The odds ratio is nearly 2, which is higher than the odds ratio for other racial groups. In addition, these ethnoracial differences are larger than those observed among the native-born population (compare columns 1 and 2, table 5). We also compare models of the infants of native-born (column 3) and foreign-born (column 4) Hispanic mothers. For the most part, the relative sizes of the estimates are similar in direction and magnitude. The largest difference is observed for mother's education attainment, whether she was a high school dropout or not. Among the newborn infants of native-born mothers who are high school dropouts, the relative risk of poverty was just 1.13 times greater than among the infants of high school graduates. The corresponding relative risk ratio among the infants of foreign-born mothers was much larger—2.65. Clearly, the infants of the less educated foreign-born Hispanic mothers face exceptionally high risks of poverty in comparison to their more educated counterparts.

Figure 1. Interaction effects between nonmetro residence and new destinations for Hispanic newborns

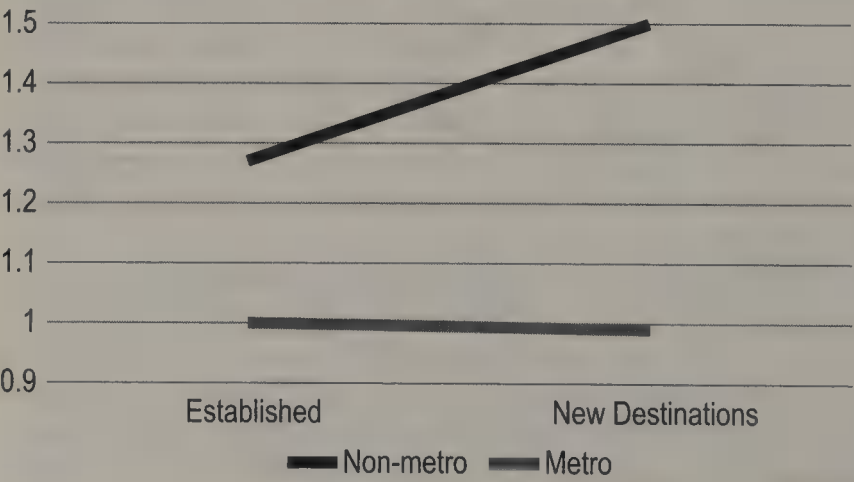


Table 5. Logistic Regression of Poverty of Newborn Infants, by Nativity Status of Mothers

	All Native born	All Foreign born	Native-born Hispanic	Foreign-born Hispanic
Variables	Odds ratio	Odds ratio	Odds ratio	Odds ratio
Household in nonmetro area	1.67***	1.26***	1.20***	1.25***
Hispanic	1.51***	1.99***	—	—
Black	1.59***	1.44***	—	—
Asian	.81**	.91	—	—
American Indian	1.35***	—	—	—
New Hispanic destination	1.17***	1.01	1.11*	1.20**
Other destination	1.01	.98	.99	.98
First birth as a teenager	1.23***	1.26***	1.29**	1.17***
Unmarried mother	12.09***	7.04***	7.13***	6.46***
Number of siblings in the household	1.37***	1.28***	1.18***	1.19***
Number of adults in the household	.65***	.67***	.68***	.72***
Migrant household	1.91***	1.35***	1.33***	1.70***
Mother with no/poor English	2.42***	2.20***	1.88***	2.27***
Mother education, high school dropout	2.53***	1.04*	1.13**	2.65***
Mother education, college graduate	.42***	.34***	.40***	.54***
Mother employed	.22***	.28***	.23***	.25***
Constant	.07	.51	.90	.42
Log likelihood	-55389.37	-17240.58	-9137.61	-10789.62
Sample size	143,268	38,159	17,927	19,215

* $p < .05$ ** $p < .01$ *** $p < .001$

A Coda on the Social "Safety Net"

From a policy standpoint, one concern is whether the families of newborn Hispanic infants are dependent on government "handouts" or whether they are "falling through the cracks" in America's welfare safety net. As a starting point for discussion, it is important to recognize that Hispanic infants born in the United States are citizens, with the same rights and obligations as other native-born populations. Table 6 provides estimates of the "dependence" of poor Hispanic newborns on government largesse, as measured by the shares using safety net programs (i.e., cash assistance and food stamps, or SNAP), along with information on the depth of poverty, as measured by average ratio of family income to the poverty threshold.

These results show that only a small fraction of poor Hispanic infants reside in families accessing government cash assistance (roughly 12 percent). The differences

Table 6. Safety Net for Poor Newborn Children, 2006–2010

	Poor Hispanic newborn households			All poor newborn households		
	Metro	Nonmetro	Total	Metro	Nonmetro	Total
Receiving welfare (%)	12.1	9.9	11.9	15.4	13.3	14.9
Receiving food stamps (%)	48.4	52.4	48.8	56.2	63.4	57.9
Average total family income (\$)	11,215	11,025	11,194	10,276	9,854	10,175
Average family income-to-poverty ratio	.46	.45	.46	.44	.44	.44

between metro and nonmetro newborns are very small (about two percentage points). This likely reflects the high shares of Hispanic newborn infants with immigrant parents, who may be unaware or ineligible for government cash programs. On the other hand, about one-half of the families of poor Hispanic newborn infants receive food stamps (or SNAP), which virtually all of these infants should be eligible to receive. Note also that for both welfare and food stamps, the proportion of Hispanic newborns receiving assistance is lower than the overall proportion of newborns receiving assistance in both metro and nonmetro areas. Clearly, these estimates uncover high levels of unmet need among poor Hispanic families.

Our results also provide little evidence that Hispanic newborns are ineligible for government benefits because they are only marginally poor. In fact, the average family income is only \$11,194 annually (over the 2006–2010 period) for poor Hispanic families including newborn infants. For comparison, median US family income in 2008 was almost six times greater—\$62,621 (see DeNavas-Walt, Proctor, and Smith 2009). The Hispanic poor at birth are deeply impoverished. The income-to-poverty ratio for poor Hispanic newborns is only .46, with almost no variation between metro and nonmetro areas. Our calculations (not shown) indicate that 18.1 percent of all Hispanic newborns are deeply impoverished (with income-to-poverty thresholds of less than .5). This compares with 12.6 percent of all newborns.

Discussion and Conclusion

The rapid growth and geographic dispersion of Hispanics raises new theoretical and empirical questions about concentrated poverty and spatially uneven patterns of economic incorporation and social integration (Lichter 2012; Parrado and Morgan 2008). As we have emphasized here, Hispanic population growth is now fueled mostly by births rather than by an influx of new immigrants. Our goal, using mother-child linked data for the first time from the ACS, has been to provide baseline estimates of poverty among Hispanic newborn infants. Our working assumption is that poverty in utero and at birth represents critical periods that shape children’s long-term cognitive development and the prospect of incorporation into American society.

The empirical results support several general conclusions. First, our estimates of the general fertility rate highlight the high fertility of Hispanics and, by implication, the pace of cultural and economic assimilation (Parrado and Morgan 2008). Our empirical approach also demonstrated the utility of the ACS fertility question, which remains an underutilized resource for monitoring racial and ethnic variation in US childbearing and economic incorporation. We showed that rates of Hispanic fertility are particularly high in rural areas, including new Hispanic destinations. Rapid population growth is now driven, at least in part, by high fertility rates among Hispanics, who are disadvantaged along many dimensions (e.g., nativity, education, etc.).

Second, our results show that poverty—like racial and ethnic diversity—starts from the “bottom up.” Long-term prospects for incorporation and upward social mobility are heavily influenced by an infant’s economic circumstances at birth. Although a question for future studies, early childhood poverty may set into motion a series of lifecycle disadvantages (e.g., inadequate parenting, bad neighborhoods, underfunded schools, poor healthcare, etc.) that culminate in poverty in adulthood. Geographic mobility patterns also may serve to concentrate poor Hispanics spatially and reduce spatial assimilation with the majority population (Foulkes and Schafft 2010). Here, we shifted the discussion to fertility by providing evidence of exceptionally high poverty among Hispanic newborns, especially in rural areas. Over 40 percent of all rural Mexican-origin Hispanic babies were born poor. Poverty among recent Hispanic newborns clearly raises the specter of new rural Hispanic ghettos and growing physical, social, and cultural isolation from the mainstream (see Burton et al. 2011). The prospect of full incorporation into American society is jeopardized.

Third, the results from the multivariate analyses indicated that high rates of poverty among Hispanic newborn babies are a product of negative selection into new destinations, as well as parenthood and demand-side factors that limit job opportunities and social support networks. Spatial isolation or concentration may also reinforce cultural expressions of Hispanicity, including familism and high fertility. Indeed, even when well-known supply-side “risk factors” (e.g., teen unmarried mothers, etc.) are taken into account, the odds of poverty among Hispanic newborns were still roughly 45 percent higher than their white counterparts. Hispanic poverty—in either rural or urban areas—cannot be reduced to simplistic explanations that emphasize maladaptive behavioral decision-making, that is, nonmarital or teen childbearing, low educational attainment, acquisition of English language skills, or other dimensions of human capital. Emerging patterns of Hispanic spatial concentration in new destinations also matter. Our analysis revealed especially large disadvantages among rural Hispanic newborns in new destinations. The substantive implication is that the lack of income from work and government (e.g., cash assistance) in new destinations is experienced disproportionately by Hispanics, even when they follow the behavioral prescriptions advocated by some politicians and pundits to “play by the rules” (i.e., get a good education, work hard, and get married before having children). Hispanics have contributed to the revitalization of dying rural communities (Carr, Lichter, and Kefalas 2012), but the payback, if measured in lower poverty rates, has been modest.

Our findings place the spotlight squarely on newly born Hispanic children. We find that newborn Hispanic infants often start life well behind the starting line, living in fast-growing boomtowns where they may never catch up. Indeed, our results highlight the need to investigate the intergenerational roots of Hispanic poverty and material hardship, and to track the developmental and economic trajectories of newborn infants in immigrant receiving areas. Failing to invest in families and children today has long-term implications that are likely to be revealed when today's disadvantaged newborns take their place (or not) in the adult world. For poor Hispanic women, new investments in rural reproductive health services, prenatal care, and nutrition programs (including SNAP and WIC) may pay large dividends in the form of healthy birth outcomes (e.g., fewer low-weight births) and better infant and child health. Unmet need is substantial in rural poor communities, which raises the specter of chronic poverty. New destinations—especially in rural areas—are too often ignored in metro-centric studies of immigrant adaptation and social integration (Lichter 2012).

Although policymakers sometimes forget, the disadvantages faced by low-wage, low-skill immigrant Hispanic workers are often most keenly felt by their US-born infants and children who, through no fault of their own, suffer the immediate and long-term consequences of low family income and concentrated poverty. We showed, for example, that the newborns of foreign-born mothers are often at greatest risk. Yet, restrictive welfare cash assistance programs and employment assistance and training programs often limit program participation among new immigrants (e.g., waiting periods). Undocumented parents (who cannot be identified in the ACS) face their own economic challenges in the workforce, including workplace exploitation and wage theft (Donato and Armenta 2011). Second-generation children—native-born US citizens—are often caught in the political crossfire. For newborn children, trajectories of cognitive and emotional development and, ultimately, full economic incorporation into American society will be shaped by the families and communities in which they live. Whether today's minority and immigrant children will assimilate into America's economic mainstream—however this is defined—is an open question that will only be answered fully in the long term, perhaps after several generations (Marrow 2013). This is a pipeline issue that will reshape America's future but also one that requires public policy attention now.

Notes

1. The Bracero period refers to a period (1942–1964) when Mexico and the United States entered into an agreement that allowed the United States to bring in temporary migrants from Mexico to work in agriculture and other manual occupations.
2. Unlike most studies of Hispanics, which include both black Hispanics and white Hispanics, Lopez and Valasco (2011) restrict their sample to Hispanics who self-identify as non-black. This means that all black Hispanics were classified as black for the purposes of their study. Since black Hispanics from the Caribbean have poverty rates that are typically lower than those of other Hispanics or blacks, classifying black Hispanics

as non-Hispanic has the effect of evaluating their estimates of “Hispanic” poverty vis-à-vis Hispanic poverty rates calculated on the basis of Hispanics of all races.

3. To illustrate this point, we examined the numerical contribution of poor Hispanics to the growth of the US poor population. In 2000, 23.0 percent of all poor people were of Hispanic origin. By 2010, this figure had increased to 29.2 percent. The population of poor Hispanics nearly doubled between 2000 and 2010, from 7.6 million to 13.5 million (Dalaker 2001; DeNavas-Walt, Proctor, and Smith 2011).
4. There is no clear consensus on how “new destinations” are or should be defined. We are limited to state data, but we have also sought to determine whether our results are robust to alternative definitions of gateways, new destinations, and residual status. For example, we replicated the analysis, while defining new destinations as including only the 10 fastest-growing states. We also limited our analyses of new destinations to states with the fastest-growing Hispanic populations (e.g., over 200 percent between 1990 and 2010), but where Hispanic represented a sizeable population (i.e., 15 states with Hispanic populations exceeding 250,000). In each case, our basic conclusions held up to these robustness checks. These results are available from the authors upon request.
5. To adjust for the clustering in the ACS data, ACS-generated personal-level replicate weights were used with all logistic regression models. This was done using the “svyset” and “svy” commands in Stata 13.
6. GFR estimates here are based on the fertility of women age 15–44 (for the 2006–2010 period). Fortunately, our ACS-based estimate of overall GFR is 67 (see table 1), which is similar to the average of the 2006–2010 rate (67.3) based on the same age group using vital registration data from NCHS (Martin et al. 2013). This speaks to the validity of ACS in reporting recent childbearing.
7. In comparison, 81.1 percent of the mothers of poor Hispanic newborn infants have low education (data not shown). These figures compare to 45.3 and 61.1 percent, respectively, of all US newborn infants (see Appendix table A1).
8. We also replicated all of our models using a multilevel modeling approach (HLM). The specific results and conclusions show little difference from those reported here (which adjust for clustering and design effects). For example, the replication of model 2 using HLM reveals a Hispanic “effect” of 3.05 (rather than 2.92, as reported in table 4, model 2). Also, the odds of poverty in rural areas is 1.44 using HLM rather than 1.51 (table 4), and the odds of poverty in fast-growing new destinations is .95 rather than .94. Our results are robust to alternative modeling approaches.
9. These results are robust to alternative definitions of new destinations. When new destinations were limited to the 10 states with the fastest-growing Hispanic populations, the OR for Hispanic poverty was even higher than those reported in table 4 (i.e., 1.98 versus 1.45).
10. This OR is 1.19 if we redefine new destinations as the 10 states with the fastest-growing Hispanic populations.
11. In some additional analysis, we also added interaction terms to model 3 between non-metro residence and new immigrant destinations. The results (OR = 1.04) indicate that, compared with metro new destinations, the odds of poverty are 1.47 times greater in new rural destinations ($1.04 \times 1.51 \times .94 = 1.47$) than in established metro destinations. Not surprisingly, the interaction effects for the total sample are smaller in magnitude than those for Hispanics (model 5); they nevertheless are large and statistically significant. One interpretation is that new destinations in rural areas pose substantial risk for poverty for all newborn children, but the largest risk for Hispanic newborn babies.

Appendix

Table A1. Parental Characteristics of All and Poor US Newborn Infants, 2006–2010

	All newborns			All poor newborns		
	Metro	Nonmetro	Total	Metro	Nonmetro	Total
Education of mother						
High school or less	43.9	51.0	45.3	61.8	59.0	61.1
Some college	21.5	29.3	22.9	32.2	37.5	33.5
College +	34.5	19.8	31.8	6.0	3.6	5.4
Age of mother						
Less than 20	6.0	8.4	6.4	12.5	14.5	13.0
20–24	18.1	27.0	19.8	31.4	38.2	33.0
25+	75.9	64.7	73.8	56.1	47.3	54.0
Marital status						
Single/never married	31.0	35.5	31.8	68.8	68.4	68.7
Married	69.0	64.5	68.2	31.2	31.6	31.3
Nativity of mother						
Native born	75.8	92.7	79.0	71.7	91.1	76.2
Immigrant	24.2	7.3	21.0	28.3	8.9	23.8
First teen birth						
First birth as a teen	16.1	21.9	17.2	32.1	35.0	32.8
First birth after teenage	83.9	78.1	82.8	67.9	65.0	67.2
English ability						
No/poor English	3.0	1.4	2.7	7.2	2.7	6.1
Good/excellent English	97.0	98.6	97.3	92.8	97.3	93.9
Employment of mother						
Employed	53.4	53.5	53.2	28.1	31.4	28.9
Unemployed	46.6	47.6	46.8	71.9	68.6	71.1
Number of siblings						
0	48.3	47.4	48.1	49.9	50.3	50.0
1	29.5	28.3	29.2	21.3	22.6	21.6
2+	22.2	24.4	22.6	28.9	27.1	28.4
Number of adults in household						
1–2	80.9	82.4	81.2	84.2	85.9	84.6
3–4	11.0	10.0	10.8	7.7	7.1	7.5
5+	8.2	7.6	8.0	8.1	7.0	7.8
Migrant household						
Migrant	23.7	25.4	24.0	37.3	38.7	37.6
Non-migrant	76.3	74.6	75.9	62.7	61.3	62.4
Geography						
Traditional gateway	52.2	20.3	46.2	54.0	21.3	46.3
New destination	25.3	55.3	30.9	25.7	54.4	32.4
Other destination	22.6	24.4	22.9	20.4	24.3	21.3

Source: American Community Survey Five-Year Sample, 2006–2010

About the Authors

Daniel T. Lichter is the Ferris Family Professor in the Department of Policy Analysis and Management, Professor of Sociology, and Director of the Cornell Population Center. His recent work has focused on changing ethnoracial boundaries, as measured by changing patterns of interracial marriage and residential segregation in the United States.

Scott R. Sanders is an Assistant Professor of Sociology at Brigham Young University. His research focuses on poverty, globalization, and healthcare. His recent publications include "Rural North versus South: The Effects of Foreign Direct Investment and Historical Legacies on Poverty Reduction in Post-Đổi Mới Vietnam," in the *Journal of Vietnamese Studies*.

Kenneth M. Johnson is Professor of Sociology at the University of New Hampshire as well as Senior Demographer at the Carsey School of Public Policy. His research examines national and regional population redistribution trends, rural and urban demographic change, and the relationship between demographic and environmental change.

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The Resurgence of Race in Spain: Perceptions of Discrimination Among Immigrants

René D. Flores, *University of Washington*

The contemporary relevance of the concept of “race” has been increasingly questioned around the world. In Europe, researchers often look with skepticism at the US emphasis on race, instead highlighting the capacity of culture, especially religion, to explain native opposition to immigrants. Using two distinct data sets, I examine self-reports of discrimination among immigrants in Spain, where elites have long denied racial differences, to understand how the reported salience of boundaries based on race, nationality, and religion change with acculturation. I find that reports of both nationality- and race-based discrimination are relatively common for newcomers, while reports of religion-based discrimination are quite rare. Yet, unlike reports of racial discrimination, reports of nationality discrimination decrease over time as immigrants’ cultural differences decline due to their cultural assimilation. For second-generation immigrants, especially non-Europeans, race replaces nationality as the primary explanation for discrimination experiences and reports of religious discrimination grow even more infrequent. I conclude that, from the perspective of immigrants, the recent transformation of Spain into a new immigrant destination has gone hand in hand with the emergence of race as the main symbolic boundary marginalizing non-European immigrants in Spain.

Introduction

The contemporary relevance of the concept of “race” has been increasingly questioned around the world. Even in the United States, where racism has been a central feature of the national experience, academics have questioned the “significance” of race (Wilson 1978) and efforts against the very collection of racial data are gaining ground (Bonilla-Silva 2006). Similarly, in Western European countries like France and Germany, the concept of “race” is generally regarded as “anathema to official thinking” (Thomson and Crul 2007, 1037) and

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only a minority of European scholars see racism motivating anti-immigrant sentiments in Europe (see Amiraux and Simon 2006; Balibar and Wallerstein 1999; Wieviorka 1992). Instead, the general consensus in European scholarship is that, though racial animosity may have been prevalent once, racism is not as significant in continental Europe as in the United States, and that cultural differences, especially religion, are the main drivers of anti-immigrant sentiment (Alba 2005; Fekete 2004; Peach and Glebe 1995; Zolberg and Long 1999).

Nevertheless, the bulk of prior research on discrimination against immigrants in Europe has been confined to a handful of societies that have received immigrant flows for decades, including Germany, France, and Great Britain (Thomson and Crul 2007). It is still unclear whether immigrants arriving in newer destinations like Southern Europe will come to perceive native opposition as being based on cultural or racial hostility (Aparicio 2007; Bail 2008).

Within this region, Spain is an especially intriguing case. In just a few years, Spain has been profoundly transformed from a country of emigration to one of the top destinations in the world for international immigrants, attracting highly diverse flows from Latin America, Eastern Europe, Africa, and Asia (Cachón Rodríguez 2003; Portes, Vickstrom, and Aparicio 2011). More importantly, though Spain is often identified as the cradle of the long-standing Western preoccupation with notions of racial purity and ancestry (Fredrickson 2002; van Dijk 2005), in the modern period Spanish political and intellectual elites, along with their French and German counterparts, have come to reject the notion of race (Molina 1994). Instead, they officially endorse a civic system of citizenship based on republican ideals (Díez Medrano 2005). In contrast, religious differences, rooted in Spain's troubled history with Muslims and Jews (Fredrickson 2002), are commonly invoked by Spanish scholars to explain native opposition to immigrants (Aparicio 2007; Morales et al. 2008). Thus, Spain is an ideal case to test whether these rapid demographic changes are activating ethnic boundaries based on cultural attributes such as nationality and religion, as many European scholars would predict, or whether race is emerging as a significant social boundary in a society that has long denied racial differences (Molina 1994).

Previous attempts to study discrimination patterns in Spain have relied on the opinions of natives—opinions that, while informative, may be distorted by social desirability bias (Bail 2008; Pettigrew 1998). Further, native opinions of minorities may primarily tap into relatively static stereotypes of different nationality groups, which may be rooted in natives' impressions of first-generation immigrants. In contrast, immigrants' own discrimination reports, which have not been systematically studied for the Spanish case, may offer precious information on immigrants' understandings of the ethnic boundaries around them and how these boundaries change as their own individual characteristics shift over time due to acculturation.

Existing theories of immigrant adaptation predict opposite outcomes for the interaction between perceptions of discrimination by immigrants and acculturation. While the classic assimilation paradigm states that discrimination should decline as immigrants assimilate culturally, conflict theorists posit that assimilated immigrants should actually perceive more discrimination as they come to perceive their actual situation of disadvantage. In this paper, I try to reconcile

these different theoretical expectations by providing a formal model of immigrant adaptation and subjective discrimination, which accounts for these different predictions. I then test this model using the case of Spain, a country with a mixed empirical record but where theorists predict culture-based discrimination to be substantially more significant than racial discrimination.

In this article, I pose the following questions: (1) What is the most prevalent reason why immigrants believe they are discriminated against in Spain: race, nationality, or religion? (2) How do discrimination reports vary by level of acculturation, generation, and when looking for work? I use two distinct data sets to address these questions: the Longitudinal Study of the Second Generation (ILSEG) and *Voz de los Inmigrantes* (VDI). The ILSEG survey was applied in 2009 to a representative sample of 6,960 school-aged immigrants in Madrid and Barcelona, two areas that host a significant portion of all immigrants in Spain. Using this data set, I conduct a systematic comparison of immigrants' perceptions of discrimination across multiple nationalities and examine whether immigrants believe they are maltreated due to cultural factors, including nationality or religion, or due to their race. I then analyze how immigrants' discrimination reports vary by level of acculturation using three distinct measures of cultural adaptation: length of residence in Spain, language skills, and nativity. Since certain ethnic boundaries may become activated only when respondents become adults and start competing with natives for scarce resources such as jobs, I analyze a second data set, VDI, which is a large-scale survey of adult first-generation immigrant job seekers conducted in five Spanish regions. VDI allows me to test whether similar results are also found among older immigrants looking for work, whether they are sensitive to question format, and whether they were also present before the 2008 Spanish economic crisis, which may have hardened public opinion toward immigrants.

Using these two data sets, I find that reports of religion-based discrimination are quite rare. Instead, nationality- and race-based discrimination are much more commonly cited by immigrants. However, while reports of nationality-based discrimination decrease with immigrants' acculturation, non-European immigrants actually report more racial discrimination with increasing acculturation. I conclude that the recent transformation of Spain into a new immigrant destination has gone hand in hand with the emergence of race as the main symbolic boundary marginalizing non-European immigrants in Spain.

Immigration and Ethnic Boundaries

Within the social sciences, there has been a long and often contentious debate about how best to conceptualize race, ethnicity, and culture (Cornell and Hartmann 1998; Omi and Winant 1994; Sewell 1999; Wimmer 2008). Here, I compare and contrast immigrants' perceptions of discrimination due to race, nationality, and religion, though I recognize that these three concepts may overlap. Indeed, from a more comprehensive Weberian framework, these three elements could all be considered special cases of "ethnicity," which can be defined as subjective feelings of belonging based on the belief in shared culture and common ancestry (Wimmer 2008).

Despite the fluidity between the conceptual borders of race, religion, and nationality, each concept is often grounded on different analytical domains (Morning 2008). Whereas race is commonly associated with perceived physical traits (Goldberg 2002; Telles and Ortiz 2009), religion and nationality are more cultural in nature, since they involve learned practices related to spiritual beliefs and geographic location and citizenship, respectively (Morning 2008). Indeed, most researchers define culture in terms of learned behavior and ritual practices (Sewell 1999; Swidler 1986).

Some scholars, particularly in the United Kingdom, often conflate race and culture, as when they refer to native opposition to immigrant cultural practices such as the wearing of headscarves in public schools or the construction of new mosques as “racism” (Fekete 2004). In this vein, European scholars have coined the term “cultural racism” to refer to the belief espoused by some European elites that immigrants have immutable cultural differences, such as religion, that are at odds with European values and culture (see Blaut 1992; Taguieff 1990; Wren 2001). However, Alba (2005, 44) argues that considering religion and other cultural practices as part of “race” would define race in “a broader way than most social scientists would.”

In this article, I take a more agnostic approach. Though I study immigrants’ reports of discrimination by race, nationality, and religion separately, I recognize that these categories have a significant conceptual overlap. By studying them separately, I seek to illuminate some of their internal dynamics and ultimately inform these larger theoretical debates. In particular, by exploring how immigrants’ usage of these categories is related to immigrants’ acculturation level, my study provides evidence for the socially constructed nature of these categories. In doing so, it could help other researchers move away from rigid conceptualizations of race, religion, and nationality, which, as Brubaker (2013) reminds us, are social divisions that are sometimes taken for granted.

Weber (1978) saw migration as a key driver of ethnic group formation. As immigrants come into contact with natives, the location and nature of ethnic boundaries between them will often be delimited through a process of acceptance or rejection of the perceived characteristics of newcomers. Thus, immigrants’ understanding of precisely why they are being rejected by natives, whether it is due to their race, religion, or nationality, could offer precious information on the *changing nature* of ethnic boundaries. This understanding could then affect their attitudes and behaviors toward the host society and even shape future political mobilization (Okamoto 2003). In turn, immigrant mobilization and experiences could affect the ethnic logic of the host society itself through their demands of state recognition of ethnic-based social cleavages (Wimmer 2008).

In addition, the nature of the native-immigrant boundary is important because it shapes the processes through which individuals gain access to privilege, status, and opportunities (Alba 2005; Bail 2008; Lamont and Molnár 2002). A bright or unambiguous boundary, often found in racialized boundaries, may narrow the possibility of full incorporation and increase the costs associated with boundary crossing. These costs include growing distance from peers, feelings of disloyalty, and concerns about acceptance. A blurry boundary, on the other hand, permits

for “ambiguous locations with respect to the boundary” and more inclusion in the mainstream (Alba 2005, 22).

Perceptions of Discrimination and Cultural Assimilation

What is the relationship between perceptions of discrimination and acculturation? In the classic assimilation paradigm, developed based on the adaptation patterns of European immigrants in the United States, native acceptance of immigrants would go hand in hand with immigrants’ cultural assimilation to the host society (Bach 1978; Warner and Srole 1945). According to this influential perspective, immigrants are resisted and discriminated against partly because of their cultural differences. However, as immigrants become more familiar with local culture and language, they become more acceptable to the native majority and, hence, less discriminated against (Portes, Parker, and Cobas 1980).

During the 1960s, an alternative theoretical perspective, often referred to as conflict theory, challenged the idea that acculturation would necessarily lead to greater acceptance and social integration for immigrants (Blauner 1972; Portes, Parker, and Cobas 1980). Inspired by the internal colonialism framework developed by Latin American scholars (González Casanova 1965), conflict theorists predicted that as immigrants acculturated into host societies, they would increasingly perceive their actual situation of disadvantage and become aware of “the reality of discrimination” (Portes, Parker, and Cobas 1980, 204). Hence, for conflict theorists, the more acculturated immigrants would perceive more discrimination against them.

More contemporary theories have stressed the multiple and contradictory trajectories that immigrant groups can follow as they adapt to the host society, particularly among the second generation (Portes and Zhou 1993; Waters 1994). Within the “segmented assimilation” framework, Portes and Zhou (1993) argue that whereas immigrants with social and economic resources will follow the linear path of assimilation traversed by earlier European immigrants, resource-deprived immigrants could encounter greater native resistance and become racialized minorities. Hence, rather than experiencing less native resistance with acculturation, this “downwardly” mobile group could actually increasingly perceive racial discrimination and develop an adversarial stance to cope with social and economic exclusion (Portes and Zhou 1993).

In her classic study of West Indian immigrants, Waters (1994) finds that immigrants’ identities are directly tied to their perceptions of discrimination and their assessments of opportunities for advancement available to them. While upwardly mobile immigrants maintain a positive outlook in life and downplay discrimination episodes, the more disadvantaged group comes to reject the immigrant aspirations of their parents and “develops perceptions of the overwhelming influence of race on their lives” (Waters 1994, 813). Hence, what changed was not necessarily the way natives treated the immigrants, but rather the cultural logic through which immigrants came to understand these experiences. Becoming “racial,” then, was related to the realization of the limited opportunities for social mobility available to people like them (Waters 1994). From this perspective, the increasing reporting of racial discrimination by immigrants reflects their

adaptation to the US context, where “race” is a primary category of difference (Roth 2012).

The Roots of Discrimination in Spain: Race or Culture?

In the United States, race is generally considered the dominant social cleavage (Bourdieu and Wacquant 1999; Heisler 2000). In contrast, European scholars, particularly outside the United Kingdom, have often questioned the validity of the concept of race in Europe (Heisler 2000).¹ In an influential article, Bourdieu and Wacquant (1999, 45–46) argue that the US experience with race is highly “particular” and that its study outside the US context is a scholarly “imposition” of the “American worldview” as the “universal point of view.” European policy-makers often look with skepticism at the alleged US “obsession” with race and have generally denounced it (Bail 2008; Lentin 2004; Taguieff 1991; Todd 1994). For example, under Republicanism in France, which emphasizes universal citizenship, even collection of racial data for social research is not allowed (Alba 2005; Amiraux and Simon 2006; Weil 2002). Similarly, in Germany, the Netherlands, and Switzerland, “race” is almost entirely absent from the political landscape, as minority groups rely instead on national identities as “identifying markers” (Koopmans et al. 2005).

While some European scholars do not deny that racism could play a role in the exclusion of immigrant groups (Balibar and Wallerstein 1999; Wieviorka 1992), researchers in Europe have more often studied new immigrant groups through the prism of culture and religion (Zolberg and Long 1999). Muslim immigrants, in particular, regardless of their country of origin, are perceived to have sharply distinct cultural values and to be less willing to assimilate (Bisin et al. 2008; Klausen 2005). Muslim immigrants’ perceived “failure to integrate,” demonstrated by their alleged participation in urban riots and terrorist attacks in Western Europe, has led many to conclude that religion, specifically Islam, is “one of the major obstacles for integration” in Europe (Thomson and Crul 2007, 1026).

Spain shares many similarities with its neighbors. This country is frequently identified as the cradle of the long-standing Western concern with ideas of race and ancestry, forged during its troubled history with Muslims and Jews (Fredrickson 2002; van Dijk 2005). Such ideas provided the legal and ideological foundation for the development of race-based caste systems in Spain’s colonial possessions around the world (Telles and Flores 2013). Nevertheless, in the modern period Spanish political and intellectual elites, along with their French and German counterparts, have come to reject the notion of race due to its biological connotations (Flecha 2001; Molina 1994). Since the practice of racial classification is often associated with “ethnic cleansing,” the Spanish government forbids officials to collect racial data (Flecha 2001), even in the case of local gypsies or *Romas*, the country’s most aggrieved ethnic minority, which is generally considered a cultural and not a racial group by State actors (Prieto-Flores and Sordé-Martí 2011). Instead, local elites have emphasized a Republican or civic form of integration to the nation during the twentieth century, which emphasizes universal citizenship as a way to overcome the racial cleavages associated with colonialism and slavery (Díez Medrano 2005; Molina 1994).

Nevertheless, Spain's recent demographic transformation has the potential to alter the local configuration of ethnic boundaries. In just a few years, Spain has been transformed from a country of emigration to a net importer of migrants (Arango 2004; Portes, Vickstrom, and Aparicio 2011). From 2000 to 2007, the foreign-born population in Spain surged from just 2.5 percent to about 12 percent of the total population (Cachón Rodríguez 2009; Morales et al. 2008). Of the total foreign-born population, about 40 percent came from Latin America (especially Ecuador, Colombia, Argentina, and the Dominican Republic), 18 percent from Morocco, 8 percent from Romania, and 5 percent from Asia (Observatorio Permanente de la Inmigración 2009).

Scholars are divided about the impact of these dramatic demographic changes on the nature of ethnic boundaries in Spain. Some authors argue that culture may be a more significant factor than race in shaping native reactions to immigrants in Southern Europe (Wieviorka 1994). This is because of the allegedly "mixed" character of the Mediterranean, which is already "marked by North African and Middle Eastern influences" (Bail 2008, 45). In a similar vein, Spanish scholars often contend that cultural affinity, and not race, is the key driver of Spaniards' treatment of immigrants. Therefore, they argue that groups that are perceived to have more cultural similarities with Spaniards, such as Latin Americans and Western Europeans, will be better received in Spain (Díez Nicolás and Lafita 2001; Escandell and Ceobanu 2009; Izquierdo, López de Lera, and Martínez 2002). In contrast, Muslim immigrants, especially Moroccans, are generally perceived to be the most culturally distinct group (Aparicio 2007).

Nevertheless, the empirical evidence is mixed. Using Spaniards' survey reports, Bail (2008) finds evidence for the salience of race as well as culture in Spain. According to the European Social Survey, which Bail draws on, 22 and 36 percent of Spaniards listed being "white" and "Christian," respectively, as significant requirements when defining their ideal immigrant. Recent qualitative work also provides mixed evidence for the salience of both racial and cultural traits in creating a sharp ethnic boundary between Spaniards and immigrants. Agudelo-Suárez et al. (2009) interviewed a Muslim woman from Morocco who complained about religious discrimination: "If you wear the headscarf, you can't work." In that same study, however, other immigrants mentioned race or color as the reason people discriminate against them in Spain. A Nigerian woman told the interviewers: "People talk to you in a certain way, they don't have any respect, I feel bad, I often think that because I'm black and come from another country, that's why they discriminate against me."

Acculturative Model of Subjective Discrimination

As mentioned earlier, existing theories of immigrant adaptation predict opposite outcomes for the interaction between perceptions of discrimination by immigrants and acculturation. Part of the reason for these differences is that these theories are making predictions about different types of discrimination. In arguing that discrimination would largely disappear with increased acculturation, the classic assimilation perspective was referring mostly to culture-based discrimination. In contrast, conflict theorists were largely making predictions about racial

discrimination. To reconcile these divergent expectations, I propose the acculturative model of subjective discrimination, which I then test using the Spanish case, a country with a mixed empirical record but where theorists predict culture-based discrimination to be more prevalent than racial discrimination.

According to the acculturative model of subjective discrimination, discrimination reports by immigrants depend on three main factors: the specific type of discrimination under study (cultural or racial), immigrants' acculturation level, and immigrants' visibility. These three factors interact to shape the likelihood that immigrants will perceive discrimination. This model predicts that, in general terms, as acculturation increases, immigrants will report less cultural discrimination. Nevertheless, as immigrants acculturate, those who are visibly distinct will increasingly report racial discrimination.

For the case of culture-based discrimination, immigrants' reports will depend on how dissimilar immigrants' culture is perceived to be from that of the majority group in the host society. More formally,

$$c = \beta_0 - \beta_1 A + \beta_2 X + \varepsilon, \tag{1}$$

where c is the probability of reporting culture-based discrimination, A is the degree of acculturation of immigrants in the host society, X is a list of covariates such as gender, age, and education that may shape discrimination reports, and ε is a disturbance term. This model predicts that reports of culture-based discrimination will decrease as the perceived cultural distance between immigrants and natives decreases.

In the case of racial discrimination, the model predicts that perceptions of discrimination will depend not only on immigrants' acculturation level, but also on their physical visibility, and on the interaction between acculturation and visibility.

$$r = \beta_0 + \beta_1 A + \beta_2 V + \beta_3 (A \times V) + \beta_4 X + \varepsilon. \tag{2}$$

In this model, acculturation (A) has a positive sign, which indicates that racial discrimination reports (r) are predicted to increase as immigrants become more acculturated in the host society. V is a measure of immigrants' physical visibility. For the case of Spain, immigrants with darker skin will be more likely to stand out. Finally, the interaction term between acculturation and visibility ($A \times V$) implies that immigrants that are more physically distinct from natives will report more racial discrimination as their acculturation increases. This model predicts that reports of racial discrimination should be high among highly visible and highly acculturated immigrants in Spain.

Highly assimilated immigrants may adopt the language of "race" to describe negative encounters with natives, since they could come to realize that despite the fact that they share many cultural attributes with natives, such as language, they still face rejection. Therefore, they may come to understand such maltreatment as being based on those elements that continue to set them apart from natives, like phenotype or race.

Data and Measurements

The first data set I analyze to test this model is the first wave of the Longitudinal Study of the Second Generation (ILSEG) in Spain conducted in 2008 in Madrid and 2009 in Barcelona. The ILSEG is an innovative data source based on a random sample of public and private schools in Madrid and Barcelona, two regions that together hosted more than 40 percent of the immigrant population in Spain in 2006 (Observatorio Permanente de la Inmigración 2009). This project specifically targeted the first three years of basic secondary school because they include the population of average age 12–17 that represented the universe of interest (Portes, Vickstrom, and Aparicio 2011). At this age, almost all children are enrolled in school, and they are sufficiently mature to be able to fill out a simple questionnaire. Hence, the survey sample is representative of the school-aged immigrant population in both regions (Portes, Vickstrom, and Aparicio 2011). The total sample size numbered 6,960: 3,375 in Madrid and 3,585 in Barcelona.

Measurement of Perceptions of Discrimination

The survey contained several questions that allow researchers to distinguish between different types of discrimination. First, respondents were asked whether they had “ever felt rejected or not treated the same as others.” Then, all respondents, regardless of their initial answers, were asked why they had been “rejected or not treated the same as others.” Respondents could choose the following non-mutually exclusive options: “I haven’t been rejected or treated in a different way from others,” “Due to my nationality,” “Due to my race,” and “Due to my religion.” A dummy variable was constructed for each of these options.

In this article, I use immigrants’ discrimination reports to explore the nature of ethnic boundaries around immigrants in Spain. A critique of this method could be that it is not entirely clear that immigrants can really know the true reason they are discriminated against. From this perspective, immigrants’ subjective understandings may not necessarily match the actual objective reasons why native Spaniards discriminate against them. Though this is a reasonable concern, a vast literature shows that ethnic boundaries are socially constructed (Barth 1969; Lamont and Molnár 2002). In other words, they depend on individuals’ impressions and beliefs. A race boundary may form if enough individuals believe that some people share some intrinsic trait such as ancestry or race regardless of the objective accuracy of this belief.

Discrimination reports are widely used in sociology and psychology (Safi 2010; Taylor and Turner 2002). Though perceptions of discrimination might not be a perfect reflection of actual experiences of maltreatment, we can expect some correspondence between the two. Indeed, some studies have found that immigrant reports of discrimination in Spain tend to correspond with native opinions toward the same immigrant groups (Morales et al. 2008). However, even if discrimination reports do not perfectly correspond with respondents’ firsthand experiences, they could still reflect their understanding of the collective experiences of members of their group (Dion and Kawakami 1996).

Discrimination episodes could be especially harmful for young students. In fact, researchers have found that discrimination experiences at schools are one of the key explanatory variables accounting for gaps in persistence and academic performance between minorities and nonminorities in the United States (Loo and Rolison 1986; Suen 1983; Tracey and Sedlacek 1987).

Measurement of Independent Variables

My key independent variable is national origin. The nationality dummies for respondents were constructed based on their country of birth in the case of first and 1.5-generation immigrants. Second-generation immigrants (those born in Spain) were assigned the country of birth of their parents. In the case of Spanish-foreign mixed marriages, respondents were assigned the country of origin of their foreign-born parent. Unfortunately, this data set does not include data on immigrants' phenotype. In this study, I use national origin as a rough proxy for immigrants' visibility, though I recognize that this is not a perfect measure. In general terms, I expect immigrants from European backgrounds, such as those originating from the Southern Cone and Eastern Europe, to be less visible in Spain than those from Asia or Africa.

I include three different measures of acculturation: years of residence in Spain, language skills, and generation since migration. A continuous variable was constructed to capture the years spent in Spain by 1.5-generation immigrants. Spanish-speaking skills were measured with a question that asked respondents how well they spoke Spanish: "little," "average," "good," or "excellent."² Immigrants that were born in a foreign country but came to Spain as children were classified as 1.5 generation, and those who were born in Spain were coded as second generation.

I also include controls for other personal characteristics that could influence perceptions of discrimination, including gender, age, and social class (Sigelman and Welch 1991; Uggen and Blackstone 2004). Socioeconomic status was measured by using the last degree completed by the respondent's mother. I also adjust my coefficients by city of residence (Madrid or Barcelona) and school type (public or non-public).

Analytic Strategy

First, I analyze the discrimination reports of 1.5-generation immigrants by running separate logistic regression models predicting the reporting of each type of discrimination (due to nationality, religion, and race). I then use multiple measures of acculturation (years of residence in Spain, language skills, and generation since migration) to test how cultural assimilation interacts with each type of discrimination. To test how language skills shape discrimination reports, I include a variable for Spanish language skills and run the model only for immigrants who come from countries where Spanish is not the native language. Next, I pool the data for all respondents for both the 1.5 and second generation, regardless of country of origin, and include a dummy variable for the second generation to estimate the impact of nativity on discrimination reports. Finally, I include national-origin interactions to test the relationship between nativity and discrimination reports

by national origin. Since my observations are nested within schools, I adjust the standard errors in all regression models for clustering at the school level. Table 1 presents descriptive statistics for all variables included in the analysis of the children sample.

Results

1.5-Generation Immigrants

Descriptive results

Only 2.9 percent of 1.5-generation immigrants report religious discrimination (though this number is relatively higher among respondents from Morocco and the Indian subcontinent).³ In contrast, nationality (17 percent) and racial discrimination (12 percent) are more commonly cited by immigrants. Immigrants from countries with significant African ancestry report the highest levels of racial discrimination, including sub-Saharan Africans (46 percent) and Dominicans (22 percent), followed by Moroccans (14 percent), Asians, and some Latin American nationalities (primarily those from Andean societies with significant indigenous populations, such as Bolivia, Peru, and Ecuador). In contrast, Eastern European groups acknowledge very little racial discrimination but report the highest levels of nationality-based discrimination, especially Romanians (40 percent).

Somewhat surprisingly, the racial discrimination results closely match reports from immigrants in the United States. According to the Children of Immigrants Longitudinal Study (CILS), a 1992 survey of 5,262 immigrants living in California and Florida, though less than 10 percent of Latin American immigrants of *mestizo* or European background reported racial discrimination, immigrants coming from societies with a significant African presence were significantly more likely to report race-based maltreatment, including the Dominican Republic (23 percent), Haiti (29 percent), Jamaica (37 percent), and the West Indies (31 percent). The only significant difference relative to the Spanish case is that in the United States very few Asian immigrants claimed racial discrimination.

Multivariate Results

Multivariate regression analyses confirm these bivariate relationships. Table 2 shows the results for logistic regression models predicting reports of racial, nationality, and religious discrimination for 1.5-generation immigrants. In models 1, 3, and 5, I include national-origin dummies along with a variable for years living in Spain. I also include a series of socio-demographic controls to account for population heterogeneity in city of residence, mother's education, gender, school type, and age (coefficients for these control variables are not reported due to space limitations). The reference category for all regression models is 1.5-generation Western European immigrants. In models 2, 4, and 6, I evaluate the impact of linguistic assimilation on experiences of discrimination, so I restrict my analysis only to respondents who came from non-Spanish-speaking countries.

Model 1 shows that Dominican immigrants have 2.5 higher odds ($\exp(1.275)$) and sub-Saharan Africans are more than eight times more likely ($\exp(2.288)$)

Table 1. Descriptive Statistics for All Respondents (*N* = 6,960)

Variable	Mean/ <i>N</i>	Std dev	Min	Max
<i>Dependent variables</i>				
Discrimination race	0.13	–	0.0	1
Discrimination nationality	0.17	–	0.0	1
Discrimination religion	0.03	–	0.0	1
<i>Key independent variables</i>				
Years in Spain	4.53	3.32	0.0	16
Spanish skills	3.60	0.69	1.0	4
2nd generation (born in Spain)	0.13	–	0.0	1
<i>National origin (N)</i>				
Western Europe ^a	266	–	–	–
Argentina	217	–	–	–
Bolivia	320	–	–	–
Brazil	113	–	–	–
Chile	96	–	–	–
Colombia	562	–	–	–
Cuban	33	–	–	–
Dominican Republic	402	–	–	–
Ecuador	1,831	–	–	–
Peru	446	–	–	–
Uruguay	61	–	–	–
Venezuela	96	–	–	–
Other Latin American	127	–	–	–
Morocco	679	–	–	–
Sub-Saharan Africa ^b	134	–	–	–
Romania	331	–	–	–
Bulgaria	92	–	–	–
Other Eastern Europe	199	–	–	–
Philippines	201	–	–	–
China	284	–	–	–
Indian subcontinent ^c	232	–	–	–
Other ^d	137	–	–	–
<i>Control variables</i>				
Barcelona	0.52	–	0.0	1
Mother's education	3.51	1.59	1.0	6
Female	0.48	–	0.0	1
Age	13.91	1.22	12.0	19

(Continued)

Table 1. *continued*

Variable	Mean/N	Std dev	Min	Max
Public school	0.82	–	0.0	1
Church attendance	2.48	1.32	1.0	5

Source: ILSEG.

^a“Western Europe” includes 22 Portuguese, 21 Italians, 17 Germans, 16 French, 11 Dutch, and others.

^b“Sub-Saharan Africa” includes 56 Equatorial Guineans, 6 Nigerians, 5 Cape Verdeans, 4 Senegalese, and other smaller groups (including 1 South African).

^c“Indian subcontinent” includes 125 Pakistanis, 43 Bengalis, and 35 Indians.

^d“Other” includes 26 Armenians, 11 US individuals, 7 Algerians, 7 Australians, and others.

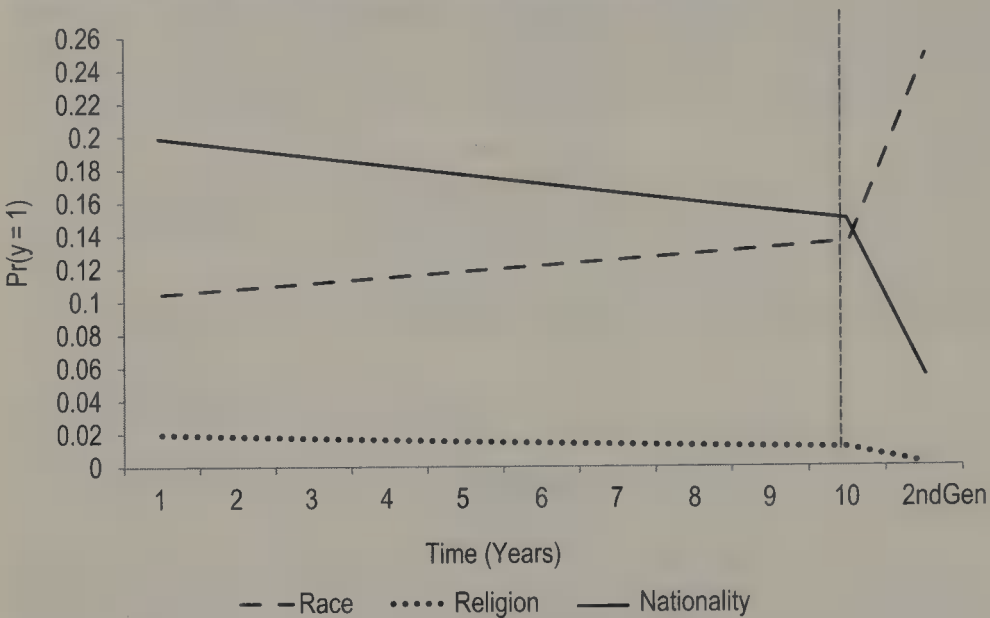
than Western European immigrants to report racial discrimination. The remaining 1.5-generation immigrants who physically stand out from the native population, such as Asians, Moroccans, and Andean Latin Americans (including many Bolivians, Peruvians, and Ecuadoreans), also report more racial discrimination, albeit not at statistically significant levels. In contrast, immigrants with European phenotypes, such as Uruguayans, Western Europeans, and Eastern Europeans, have the lowest likelihood to report discrimination due to their race.

On the other hand, the results in model 3 for the logistic regression predicting reports of discrimination due to nationality, also in table 2, indicate that Romanian immigrants have 2.2 higher odds of reporting discrimination than Western Europeans due to their nationality ($\exp(1.183)$). Similarly, the coefficient for other Eastern European nationalities, which includes Russians, Poles, and Ukrainians, is also marginally significant ($p = .091$) and positive (.504).

To calculate the equation for religious discrimination, I include a control for church/mosque attendance to take into account potentially different levels of religiosity that could impact the report of discrimination. Model 5 shows that, as expected, only immigrant youth from heavily Muslim regions, including Moroccans and Pakistanis, report significantly higher levels of religious discrimination, suggesting that the salience of religion as a social boundary is present almost exclusively for Muslim immigrants.

As predicted by my formal model, the variable for years of residence in Spain has sharply different results by type of discrimination. As models 1, 3, and 5 show, for the average first-generation immigrant, each year spent in Spain reduces the odds of reporting nationality by 3.5 percent ($1 - [\exp(-0.035)]$) and religious discrimination by 6 percent ($1 - [\exp(-0.063)]$), respectively, but it increases the odds of reporting racial discrimination by 3 percent ($\exp[0.030]$). Comparing the coefficients of groups in logistic models is a complex endeavor because conventional tests of equality confound the magnitude of the regression coefficients with unexplained variation (Allison 1999). Hence, I report predicted probabilities since they are unaffected by residual variation (Long 2009). Figure 1 shows the predicted probabilities of experiencing each type of discrimination by length of residence and by generation (comparing 1.5- and second-generation immigrants). The probabilities for nationality and race are expected to converge at year 12 of residence in Spain, suggesting that, for the average 1.5-generation immigrant,

Figure 1. Predicted probabilities of reporting different types of discrimination over years spent in Spain (1.5-generation) and for second-generation respondents



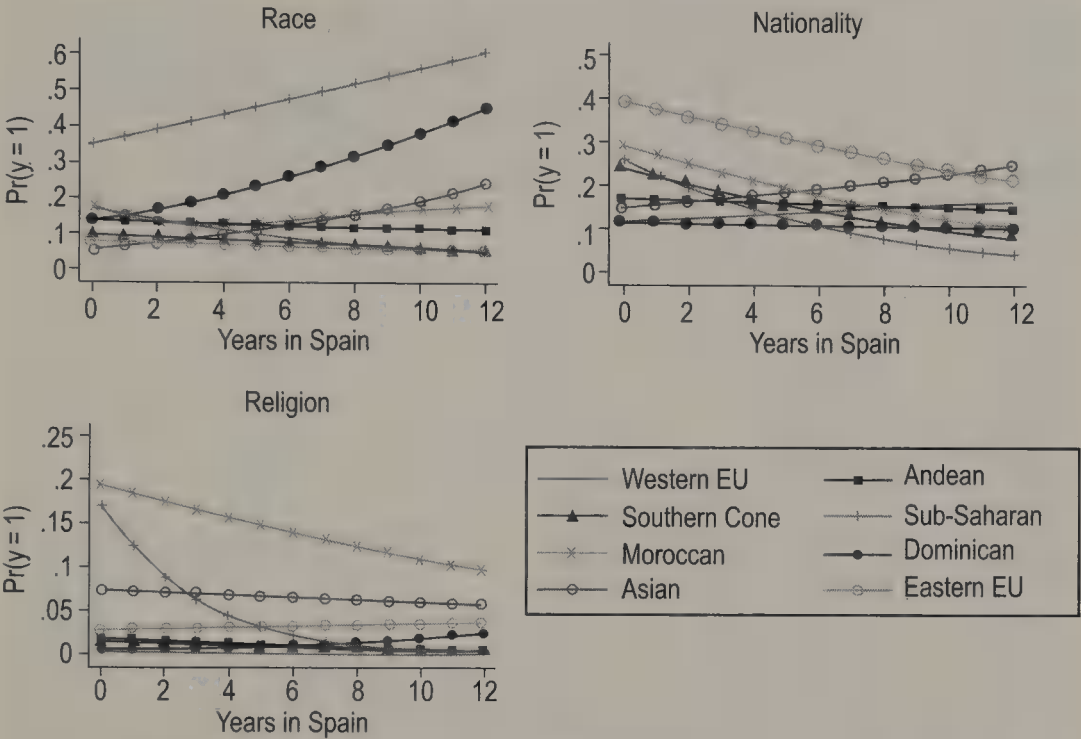
Source: ILSEG's children sample. Estimates for the statistical effect of time in Spain were calculated only for 1.5 generation immigrants (logistic model in table 2). Estimates for the second generation were based on models from table 3. Hence, all estimated probabilities were adjusted for individual age, city of residence, sex, mother's education, and school type. The reference categories consist of 1.5 and second generation Western European immigrants, respectively. Other reference categories are male, Madrid & Private School

race consolidates over time as a primary explanation for her discrimination experiences.

In figure 2, I explore how the probability of reporting each type of discrimination over time varies by each national-origin group.⁴ This figure shows that the probabilities of reporting racial discrimination are predicted to increase over time for most non-European groups. This increase is statistically significant ($p = 0.001$) and particularly steep for Dominican immigrants, who have a 46 percent probability of reporting racial discrimination when residing in Spain for 12 years compared to 13 percent for newly arrived Dominicans. Statistically significant increases over time are also found for sub-Saharan Africans, Asians, Indians, and Moroccans. In contrast, immigrants with European phenotypes such as Eastern Europeans and Southern Cone immigrants (Argentineans, Uruguayans, and Chileans) tend to report little racial discrimination, and such reporting remains quite low among long-term residents. Andean immigrants occupy a middle position in the probability of reporting racial discrimination, though it is also predicted to decline over time.

In contrast, figure 2 shows that for most 1.5-generation immigrants, the probability of reporting nationality discrimination is expected to decline over time (with the sole exception of Asian immigrants). This decline is particularly pronounced for sub-Saharan Africans, going from 26 percent for newly arrived immigrants to less than 5 percent for immigrants with 12 years of residence in Spain ($p = 0.001$). Similar rapid decreases are also predicted for Moroccan, Southern Cone, and Eastern European immigrants, but with one crucial differ-

Figure 2. Predicted probabilities of reporting each type of discrimination by national origin and years of residence in Spain among 1.5-generation immigrants



Source: Longitudinal Study of the Second Generation (ILSEG), children sample. Based on separate regression models shown on table 2, which include controls for age, sex, city of residence, Mother’s Education, and school type.

ence. Unlike immigrants with non-European phenotypes, Southern Cone and Eastern European immigrants are also predicted to report less racial discrimination over time, suggesting that their European appearances can make them invisible in the Spanish context as they become more acculturated. In contrast, as anticipated by my formal model, the regression models predict that non-European groups with more years of residence shift away from “nationality” and toward “race” as the primary explanation for their discrimination experiences.

Similarly, figure 2 shows that the probability of reporting religious discrimination is expected to decline or remain stable for all groups. This decline is found even in the case of Moroccan immigrants, whose probability of reporting religious discrimination declines from 21 percent for recently arrived individuals to 9 percent for immigrants with 12 years of residence in Spain ($p < .10$). This evidence indicates that although the salience of religion as a source of exclusion is greater for Moroccan immigrants than it is for most groups, its importance is substantially lower among long-term residents in Spain.

Language Skills and Discrimination

These findings by national origin appear to match the trends predicted by the acculturative model of subjective discrimination: as immigrants spend time in Spain, the cultural distance between newcomers and natives decreases, resulting in lower levels of perceived culture-based discrimination, whether by religion or

Table 2. Logistic Regression Predicting Reporting Discrimination Due to Race, Nationality, and Religion among 1.5-Generation Immigrants

Variables	Race		Nationality		Religion	
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Years in Spain	0.030* (0.013)	0.0416+ (0.025)	-0.035** (0.012)	-0.056* (0.025)	-0.063* (0.028)	-0.028 (0.039)
Spanish skills		0.212* (0.104)		-0.189+ (0.103)		-0.235+ (0.136)
Argentina	0.027 (0.369)		0.303 (0.317)		-0.109 (1.210)	
Bolivia	0.550 (0.413)		-0.118 (0.298)		-0.485 (1.244)	
Brazil	0.191 (0.534)	0.399 (0.547)	0.634 (0.325)	0.411 (0.449)	1.215 (1.075)	1.270 (1.086)
Chile	0.051 (0.489)		-0.629 (0.473)			
Colombia	0.551 (0.382)		0.312 (0.260)		-0.118 (1.100)	
Cuba	0.168 (0.720)		0.342 (0.516)			
DR	1.275*** (0.362)		-0.404 (0.268)		-0.347 (1.156)	
Ecuador	0.528 (0.366)		0.022 (0.257)		-0.197 (1.043)	
Peru	0.377 (0.390)		0.274 (0.275)		-0.983 (1.258)	
Uruguay	-0.796 (0.733)		-0.0718 (0.444)			
Venezuela	-0.551 (0.552)		-0.0195 (0.354)			
Morocco	0.602 (0.384)	0.731 (0.396)	0.229 (0.272)	-0.0207 (0.367)	2.638* (1.044)	2.634* (1.048)
Sub-Saharan Africa	2.288*** (0.363)	2.449*** (0.416)	-0.192 (0.410)	-0.485 (0.553)	1.118 (1.129)	1.204 (1.153)
Romania	-0.007 (0.421)	0.101 (0.466)	1.183*** (0.267)	0.945** (0.309)	1.070 (1.057)	1.138 (1.061)
Bulgaria	-0.630 (0.603)	-0.486 (0.631)	0.526 (0.374)	0.422 (0.451)	-0.002 (1.435)	-0.0726 (1.431)

(Continued)

Table 2. *continued*

Variables	Race		Nationality		Religion	
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Other Eastern Europe	-0.061 (0.395)	0.044 (0.423)	0.504 (0.297)	0.431 (0.368)	0.308 (1.166)	0.304 (1.151)
Philippines	0.355 (0.521)	0.591 (0.567)	-0.0719 (0.330)	-0.239 (0.492)	0.990 (1.174)	1.079 (1.194)
China	0.347 (0.416)	0.788+ (0.467)	0.335 (0.323)	0.090 (0.469)	0.056 (1.244)	-0.282 (1.265)
Indian subcontinent	-0.036 (0.488)	0.080 (0.498)	-0.056 (0.323)	-0.014 (0.460)	2.427* (1.047)	2.425* (1.048)
Other	0.337 (0.495)	0.559 (0.487)	0.009 (0.470)	-0.037 (0.631)	0.920 (1.173)	1.078 (1.179)
Constant	-3.295*** (0.711)	-3.691** (1.259)	-2.844*** (0.577)	-1.676+ (0.930)	-5.906*** (1.455)	-4.844** (1.690)
Observations	5,989	1,994	5,988	1,999	5,401	1,823
Pseudo R-squared	0.0365	0.0748	0.0386	0.0497	0.190	0.137

Source: ILSEG's children sample.

Note: Robust standard errors in parentheses (adjusted for clustering at the school level). Models control for age, gender, city of residence, mother's education, and school type. Religious models also control for church attendance. Reference categories are Western Europe, male, Madrid, and private school. Log odds reported.

*** $p < 0.001$ ** $p < 0.01$ * $p < 0.05$ + $p < 0.10$

nationality, but in higher levels of racial discrimination for non-European immigrants. That said, the statistical effect of years of residence in Spain could also be driven by processes other than cultural assimilation. For example, this variable could be capturing immigrants' degree of exposure to Spanish society. An immigrant with more years of residence may have a higher likelihood of experiencing discrimination due to her having more "opportunities" to be discriminated against regardless of degree of acculturation. In addition, time in Spain might not necessarily imply a greater acquisition of Spanish culture and language. This is especially the case for relatively isolated immigrant groups living in strong co-ethnic communities.

To address these concerns, in this section I employ Spanish language skills, a commonly used measure of acculturation (Telles and Ortiz 2009; Tran 2010), to test the robustness of my results. In models 2, 4, and 6, I eliminate from my sample all immigrants who come from countries where Spanish is the official language. This means eliminating all Latin American immigrants with the exception of Brazilians. I then run logistic regression models predicting reports of each type of discrimination, and I include an indicator for Spanish language skills based on self-reports.

Model 2 shows that the indicator for Spanish-speaking skills follows the same pattern as years of residence in Spain. Even after adjusting for years of residence in Spain, Spanish language skills are associated with a significant increase in reporting racial discrimination and a decline in reporting discrimination due to nationality and religion ($p < .10$). More specifically, immigrants who speak excellent Spanish have 21 percent lower odds of reporting religious discrimination ($1 - [\exp(-0.235)]$), 17 percent lower odds of claiming nationality discrimination ($1 - [\exp(-0.189)]$), but 23 percent *higher* odds of reporting racial discrimination ($\exp(0.212)$), holding all other variables in the model constant.

Second-Generation Immigrants

Yet another way to test how acculturation interacts with cultural and racial discrimination is by comparing discrimination reports across generations. Since they are born in Spain, we would expect second-generation immigrants to be more culturally assimilated than 1.5-generation immigrants. How might this impact their own perceptions of discrimination?

Among the Spanish-born second generation, nationality-based discrimination nearly disappears and religious discrimination remains rare (only 4 and 3 percent report it, respectively). Even Moroccan immigrants exhibit a statistically significant decline in reporting religious discrimination in the second generation (10 percent) compared to their first-generation counterparts (14 percent). In contrast, discrimination due to race experiences a statistically significant increase, to 15 percent, and is the most common form of discrimination among the second generation, affecting 51 percent of Chinese-origin youth, 40 percent of sub-Saharan-origin immigrants, and 23 percent of Dominican-origin youth.

Table 3 shows the results for the logistic regression models predicting discrimination reports by 1.5- and second-generation immigrants. In models 1, 3, and 5, I pool 1.5- and second-generation immigrants together and include an indicator for the second generation. These results confirm trends found for the 1.5 generation. While nativity in Spain is a significant predictor of a decrease in the odds of reporting religious and nationality discrimination, it is associated with an increase in the odds of reporting discrimination due to race. In models 2, 4, and 6, I test for differences by national-origin group by introducing interaction terms of second generation by national origin. In accordance with my previous results, the coefficients for the interaction terms suggest that most non-European second-generation immigrants report more racial discrimination than their 1.5-generation counterparts. In contrast, not a single second-generation Argentinean or Eastern European claimed racial discrimination, which led to the dismissal of their interaction terms by the statistical software due to perfect prediction. Just like their 1.5-generation co-ethnics, second-generation sub-Saharan Africans and Dominicans continue to report high levels of racial discrimination, but now are joined by most other non-European groups (including Asians, Moroccans, and Indians). Interestingly, I find that Peruvian immigrants, who constitute the only indigenous/*mestizo* group included in this model, are the only non-European immigrants who do not exhibit a statistically significant higher probability of experiencing racial discrimination in the second generation. In the case of nationality-based discrimination,

Table 3. Logistic Regression Predicting Reporting Discrimination Due to Race, Nationality, and Religion among 1.5- and 2nd-Generation Immigrants

Variables	Race		Nationality		Religion	
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
2nd generation	0.344* (0.142)	-0.907 (0.471)	-1.561*** (0.177)	-2.105*** (0.679)	-0.687** (0.223)	-0.964* (0.442)
Argentina	0.257 (0.334)	-0.248 (0.336)	0.472 (0.291)	0.321 (0.318)	0.423 (1.222)	0.329 (1.239)
DR	1.671*** (0.319)	0.985** (0.302)	-0.219 (0.258)	-0.390 (0.269)	0.193 (1.158)	0.101 (1.183)
Peru	0.762* (0.342)	0.090 (0.341)	0.423 (0.264)	0.291 (0.276)	-0.422 (1.253)	-0.523 (1.279)
Other Latin American	0.876** (0.312)	0.184 (0.291)	0.231 (0.243)	0.087 (0.254)	0.480 (1.035)	0.369 (1.074)
Morocco	1.073*** (0.318)	0.350 (0.326)	0.326 (0.248)	0.236 (0.273)	3.262** (1.032)	3.115** (1.083)
Sub-Saharan Africa	2.648*** (0.315)	1.976*** (0.291)	-0.178 (0.397)	-0.179 (0.405)	1.558 (1.122)	1.488 (1.138)
Eastern Europe	0.227 (0.322)	-0.424 (0.312)	1.045*** (0.255)	0.919*** (0.270)	1.430 (1.046)	1.322 (1.077)
Philippines	0.921* (0.367)	0.115 (0.484)	0.373 (0.286)	-0.043 (0.330)	1.387 (1.151)	1.340 (1.160)
China	1.312*** (0.327)	0.092 (0.364)	0.593+ (0.314)	0.340 (0.326)	0.567 (1.247)	0.472 (1.265)

(Continued)

Table 3. continued

Variables	Race		Nationality			Religion	
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	
Indian subcontinent	0.524 (0.430)	-0.258 (0.426)	0.143 (0.322)	-0.048 (0.325)	3.002** (1.044)	2.906** (1.086)	
Arg × 2ndGen				0.615 (1.117)			
DR × 2ndGen		1.065* (0.485)		0.924 (0.927)			
Peru × 2ndGen		0.858 (0.644)		0.143 (1.012)			
Latin × 2ndGen		0.684 (0.612)		0.460 (0.854)			
Morocco × 2ndGen		1.317* (0.516)		0.056 (0.850)		0.402 (0.520)	
Sub-Saharan × 2ndGen		1.109* (0.534)					
Eastern EU × 2ndGen				-0.742 (1.261)			
Philippines × 2ndGen		1.399* (0.643)		1.440 (0.821)			
China × 2ndGen		3.351*** (0.597)		1.673* (0.816)			

Indian × 2ndGen	1.647*	1.255	0.035
	(0.704)	(1.004)	(0.868)
Constant	-3.138**	-2.774**	-5.923***
	(0.643)	(0.564)	(1.487)
Observations	6,897	6,883	6,533
Pseudo R-squared	0.041	0.051	0.186

Source: ILSEG's children sample.

Note: Robust standard errors in parentheses (adjusted for clustering at the school level). All models include controls for age, gender, city of residence, mother's education, and school type. Religious model also controls for church attendance. Reference categories are Western Europe, male, Madrid, and private school. Log odds reported.

*** $p < 0.001$ ** $p < 0.01$ * $p < 0.05$ + $p < 0.10$

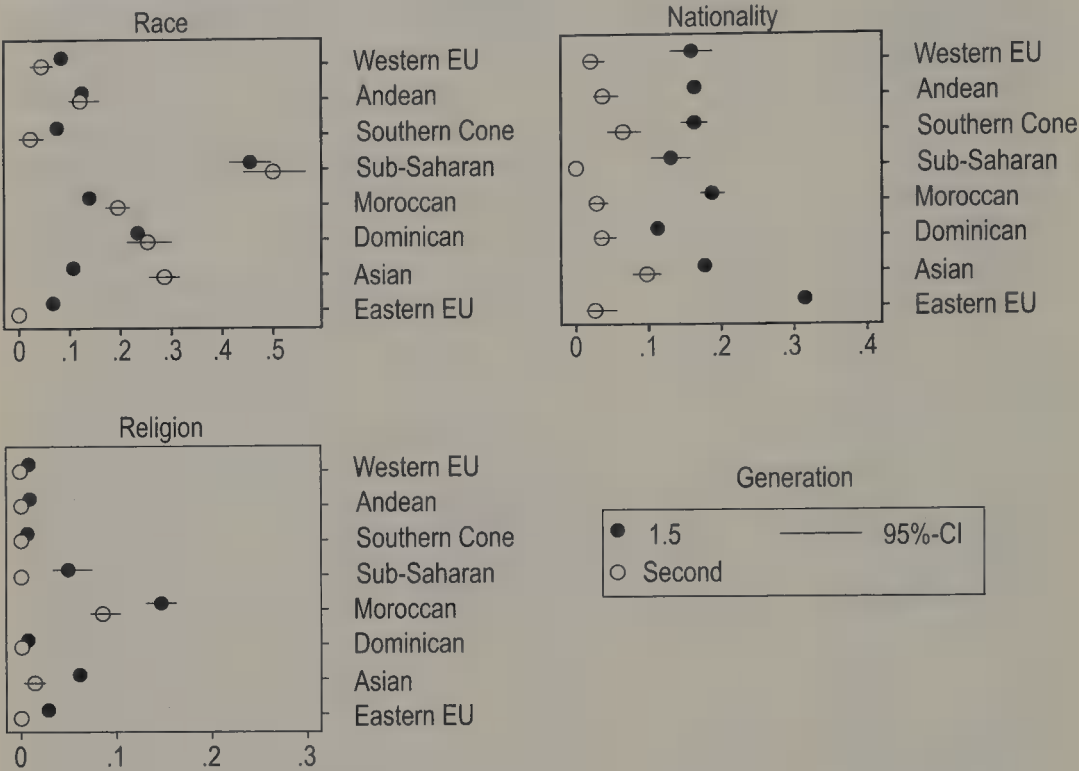
model 4 indicates that only Chinese-origin respondents are more likely to report nationality-based discrimination in the second generation. In contrast, the regression results show that second-generation Eastern Europeans no longer report more nationality-based discrimination than their Western-European counterparts. Descriptively, while 37.74 percent of all 1.5-generation Eastern Europeans report nationality discrimination, only 3 percent of their second-generation counterparts do so.

Results from the logistic regression models predicting religious discrimination also mirror previous results. Since most second-generation immigrants do not report religious discrimination, interaction terms could be included only in the case of immigrants from Morocco and the Indian subcontinent. Figure 3 summarizes these results by showing the predicted probabilities of reporting each type of discrimination for 1.5- and second-generation immigrants for the national-origin groups that had sufficient cases in both generations.

Perceptions of Discrimination among Adult Immigrants

Researchers have argued that natives' perceived competition over economic resources with foreign-origin workers could shape public attitudes toward immigrants (Citrin et al. 1997; Mayda 2006). Therefore, one limitation of using reports by school-aged immigrants is that many of them have not attempted to join the formal labor market yet. This could affect their discrimination reports, since certain ethnic boundaries may become activated only when respondents begin competing with natives for scarce resources such as jobs. In a similar vein, another concern is that since the ILSEG survey was conducted in the midst of Spain's 2008 financial crisis, characterized by a sharp increase in the country's unemployment rate, immigrants' discrimination reports may

Figure 3. Predicted probabilities of reporting each type of discrimination by national origin and generation in Spain



Source: Longitudinal Study of the Second Generation (ILSEG), children sample. Estimates are based on logistic models from Table 3, adjusted for age, city of residence, sex, mother’s education, and school type. The reference categories are: Western Europe, first-generation, male, Madrid, & Private School. “Asian” includes immigrants of Chinese, Filipino, and Indian ancestry. “Southern Cone” includes immigrants of Argentinean, Chilean, and Uruguayan ancestry

have reflected a recent deterioration of Spanish public opinion toward immigrants. To address these concerns, I turn to a second data set, “La Voz de los Inmigrantes” (VDI), a series of surveys of first-generation immigrants in Spain collected between 2000 and 2004, in the midst of a rapid expansion of Spain’s economy (Díez Nicolás and Lafita 2001).⁵ The VDI interviewed 3,048 immigrants age 16 and over in the five Spanish regions with the largest immigrant concentrations: Catalonia, Madrid, Canary Islands, Andalucía, and Valencia.⁶ Crucially, the survey asked immigrants whether they have had difficulties finding a job.⁷ Survey takers then asked the 2,805 immigrants who responded affirmatively a follow-up open-ended question (no response categories were provided to them): “What do you think is the main reason why you have had difficulties finding a job when you have searched for one?”

The VDI data largely confirm my previous findings (based on ILSEG data). While 10 and 9 percent of respondents believed that their “nationality” and their “race,” respectively, were the main reasons they had difficulty finding a job, less than 1 percent of job seekers believed that “religion” had been their main obstacle (including only 1.2 percent of Moroccans). To analyze national-origin differences in reporting each type of discrimination in a systematic way, I use a multinomial logistic model to adjust for population heterogeneity. In this model,

Table 4. Descriptive Statistics ($N = 3,048$), *Voz de los Inmigrantes*

Variable	Mean	Std dev	Min	Max
<i>Dependent variables</i>				
Discrimination race	0.09	–	0.0	1
Discrimination nationality	0.10	–	0.0	1
Discrimination other	0.53	–	0.0	1
No discrimination	0.28	–	0.0	1
<i>Key independent variables</i>				
Less than 6 months	0.14	–	0.0	1
6–12 months	0.16	–	0.0	1
1–2 years	0.22	–	0.0	1
2–5 years	0.27	–	0.0	1
5+ years	0.20	–	0.0	1
<i>National origin</i>				
Andean Region	0.15	–	0.0	1
Colombia	0.04	–	0.0	1
Cuba	0.04	–	0.0	1
D.R.	0.04	–	0.0	1
Other Latin American	0.02	–	0.0	1
Eastern Europe	0.06	–	0.0	1
Asian	0.06	–	0.0	1
Morocco	0.31	–	0.0	1
Sub-Saharan Africa	0.15	–	0.0	1
India	0.03	–	0.0	1
Other	0.03	–	0.0	1
<i>Control variables</i>				
Andalucia	0.20	–	0.0	1
Canary Islands	0.20	–	0.0	1
Catalonia	0.20	–	0.0	1
Valencia	0.16	–	0.0	1
Madrid	0.20	–	0.0	1
Murcia	0.03	–	0.0	1
Female	0.43	–	0.0	1
Age	31.50	8.59	16	90
Education	4.26	1.64	1.0	7
2001	0.25	–	0.0	1
2002	0.25	–	0.0	1
2003	0.25	–	0.0	1
2004	0.25	–	0.0	1

Source: *Voz de los Inmigrantes* Study 2000–2004 (VDL).

Table 5. Multinomial Logistic Regression Predicting Reporting Discrimination When Searching for a Job Due to Race, Nationality, and Other Reasons among First-Generation Adult Immigrants in Spain

Variables	(1) Nationality	(2) Race	(3) Other
<i>National origin</i>			
Andean	-0.620 (0.368)	1.220 (1.076)	-0.125 (0.224)
Colombia	0.151 (0.456)	1.716 (1.181)	0.271 (0.290)
Cuba	-0.106 (0.423)	1.320 (1.184)	-0.108 (0.303)
Dominican Republic	-0.249 (0.527)	-12.26*** (1.036)	-0.157 (0.293)
Other Latin	0.317 (0.565)	1.759 (1.274)	-0.164 (0.487)
Eastern European	-0.074 (0.419)	1.473 (1.171)	0.220 (0.260)
Asia	-1.359** (0.460)	2.130* (1.067)	-1.396*** (0.276)
Morocco	0.753* (0.315)	3.601*** (1.039)	0.379 (0.216)
Sub-Saharan Africa	0.110 (0.381)	4.177*** (1.042)	0.676** (0.244)
India	-0.251 (0.566)	2.310* (1.161)	0.0962 (0.341)
<i>Time in Spain</i>			
6-12 months	0.107 (0.294)	0.458 (0.300)	0.010 (0.196)
1-2 years	-0.092 (0.273)	-0.092 (0.293)	-0.526** (0.177)
2-5 years	-0.211 (0.279)	-0.161 (0.298)	-0.579*** (0.175)
5-10 years	-0.353 (0.322)	-0.276 (0.332)	-1.059*** (0.201)
More than 10 years	-1.111** (0.414)	-0.210 (0.368)	-1.384*** (0.236)
Constant	-0.581 (0.485)	-2.956** (1.119)	1.229*** (0.302)

(Continued)

Table 5. *continued*

Variables	(1)	(2)	(3)
	Nationality	Race	Other
Observations	2,743	2,743	2,743
Pseudo R-squared	0.105	0.105	0.105

Source: *Voz de los Inmigrantes* Study 2000–2004 (VDL).

Note: Robust standard errors in parentheses. Models include controls for sex, age, educational attainment, region, and study wave. Reference categories are Southern Cone immigrants, male, living in Spain for less than 6 months, and Andalusia.

*** $p < 0.001$ ** $p < 0.01$ * $p < 0.05$

I regress the reporting of nationality and racial discrimination on national origin and adjust for differences in age, sex, educational attainment, years living in Spain, Spanish region, and study wave.⁸ Because only 11 respondents reported religious discrimination, I had to include these cases in an “other” category, which also includes other reasons such as lack of contacts, lack of education, and so forth. The reference outcome is those individuals who did not encounter any obstacle when looking for a job. I use Southern Cone immigrants as the comparison group because they report little discrimination and because Western European immigrants were not available in this study.

Mirroring my previous results, I find that with the exception of other Latin Americans and Eastern Europeans, all immigrant groups, including Asians, sub-Saharan Africans, Indians, and Moroccans, report significantly more racial discrimination when searching for a job than do Southern Cone respondents. The results for “nationality” discrimination are somewhat more divergent than my ILSEG findings, with Asians reporting less discrimination and Moroccans more discrimination than Southern Cone immigrants. However, time spent in Spain has a similar statistical effect in the VDI sample. Whereas the predicted probabilities of reporting nationality-based discrimination shrink by half after a residency of more than 10 years, the probability of encountering “racial” discrimination does not diminish over time.

Overall, these results largely confirm that the discrimination patterns I found among young immigrants are also present among older immigrants who are searching for jobs, for whom “religion” serves as a weak ethnic boundary, while “nationality” and “race” are perceived as greater obstacles (the latter affecting mostly non-European immigrants from Asia and Africa). Furthermore, the open-ended nature of this question provides evidence that my results are not driven by the categories included in the ILSEG questionnaire but that older immigrants spontaneously report them in similar patterns.

Discussions and Conclusions

This article makes several significant contributions to our understanding of immigrant adaptation patterns and ethnic boundaries in Europe. First, contrary to the conclusion of a substantial part of the literature that religion is the most

significant ethnic boundary around immigrants in Western Europe (Alba 2005; Goldberg 2006; Lamont 2000; Peach and Glebe 1995; Thomson and Crul 2007; Zolberg and Long 1999), I find that very few immigrants in Spain, including Moroccans, believe religious discrimination plays a significant role in their lives. Second, I find that, instead of religion, a significant number of immigrants believe that both their nationality and their race are the key factors driving their maltreatment by others in Spain. Nevertheless, unlike racial discrimination, reports of nationality discrimination are predicted to decrease over time as immigrants' cultural differences decline with acculturation. Third, I find that cultural assimilation interacts with immigrants' phenotypes to shape the nature of ethnic boundaries around immigrants in Spain. While some European-origin immigrants, such as Eastern Europeans, perceive high levels of nationality discrimination, they stop reporting it as they learn the Spanish language and spend time in Spain. This suggests that their European phenotype allows them to become virtually invisible as they acculturate. In contrast, as non-European immigrants, including Asians, Indians, Dominicans and sub-Saharan Africans, assimilate into Spanish culture, their understanding of the reason for their maltreatment seems to shift away from culture and toward race. Indeed, among their second generation, race overtakes nationality as the primary reason that immigrants cite for discrimination. I conclude that cultural assimilation goes hand in hand with the increasing significance of race as the main symbolic boundary marginalizing non-European immigrants in Spain.

Hence, the case of Spain shows that although Western European intellectual and political elites refuse to recognize the allegedly controversial concept of "race," due to their emphasis on universal models of citizenship, many immigrants in Spain are increasingly describing the boundaries that separate them from native Spaniards and the social conditions they are encountering as "racial." In doing so, my findings suggest that categories of difference, like race, may emerge from the bottom up as immigrants utilize them to make sense of their experiences even if these categories are not formally available in the official ethnic repertoire of a given nation. These findings call into question the validity of the universalistic model for the case of Spain. They also provide empirical evidence that the significance of "race" to the process of immigrant incorporation is not just a scholarly imposition of the American worldview as the universal point of view, as Bourdieu and Wacquant (1999) have argued, but instead is a term utilized by immigrants in Europe to make sense of their experiences.

My findings provide mixed evidence for immigrant adaptation theories. As the classic assimilation theory predicted, I find that highly acculturated immigrants report less cultural discrimination. Nevertheless, I also find that, as the conflict theory posited, immigrants with non-European roots do report more racial discrimination over time. Therefore, rather than helping us adjudicate among competing theories, my findings reveal these theories' complementarity. Under certain scope conditions, both of these theories make correct predictions.

As a way to identify these conditions, I propose the acculturative model of subjective discrimination, which attempts to reconcile these seemingly divergent outcomes predicted by these earlier theories. According to this model, the conditions that shape immigrants' perceptions of discrimination include the specific

type of discrimination under study (cultural or racial), immigrants' acculturation level, and immigrants' visibility. This model predicts that, generally speaking, as acculturation increases, immigrants will report less cultural discrimination. Nevertheless, as immigrants acculturate, those who are visibly distinct will increasingly report racial discrimination. I find that this formal model captures empirical data patterns from the Spanish case relatively well.

In addition, this study challenges some of the prevailing wisdom regarding national-origin differences in discrimination experiences in Europe. Researchers looking to explore issues of discrimination and prejudice tend to focus on African and especially Muslim immigrants, based on evidence from other European societies, including Germany, the Netherlands, and France (Alba 2005; Peach and Glebe 1995). However, I find that Moroccan immigrants are not the group most discriminated against in Spain. Instead, they generally occupy an intermediate position compared to other groups. This finding could qualify some of the pessimism among scholars regarding the future of Moroccan immigrants in Spain (Aparicio 2007; Morales et al. 2008).

I find that Romanians are the group most discriminated against due to nationality in the first generation, but they stop reporting discrimination over time as they acculturate. Does this mean that the ethnic category of "Romanian" is less stigmatized over time in Spain? Not necessarily. Spaniards might still have very negative opinions toward "Romanians," but due to their European phenotype, it might be increasingly difficult to correctly classify who "belongs" in the "Romanian" category as Romanians acculturate. Hence, my study highlights the importance of analyzing immigrant reports, in addition to native opinions, to understand the changing nature of ethnic boundaries around immigrants.

The case of Spain could shed light on the experiences of other Western European societies, such as Italy and Portugal, that are also undergoing rapid demographic changes. Just like in these countries, many scholars in Spain expected religious and other cultural differences to be the main drivers of nativism. At the same time, Spain has unique characteristics. Unlike other countries of Europe, to this day no mainstream Spanish political party has endorsed an extremist anti-immigrant platform. The political promotion of cultural cleavages by political actors could activate culture-related concerns about immigration. In addition, Spain has long-standing colonial and cultural links with many immigrant-sending countries. Such historical links may be facilitating the relatively benign experiences reported by Latin American immigrants (with the exception of Dominicans).

Based on these findings, I hypothesize that European and Southern Cone Latin Americans could become rapidly integrated into Spanish society, achieving virtual invisibility by the second generation. A middle group may be formed by Moroccans and Latin Americans of *mestizo* and indigenous backgrounds. The three groups that could become racialized minorities in Spain are Asians, Afro-origin individuals, and immigrants from the Indian subcontinent. These immigrants' realization of the racial nature of the boundary around them was evident in the open-ended comments of survey respondents. For example, a 13-year-old Dominican immigrant who has lived in Barcelona for four years offers this explanation for his discrimination experiences in Spain: "I think because I'm a person of color." This development may spell trouble for second-generation immigrants

in Spain because race-based boundaries are especially unambiguous (or “bright”) and “virtually uncrossable” for some (Alba 2005, 37). Indeed, overcoming a race-based symbolic boundary might require a profound society-wide boundary shift: the redefinition of “Spanish” to also include non-Europeans.

These developments could also have important political consequences. “Pan-ethnicity” scholars have argued that individuals who are subjected to common experiences such as oppression and discrimination may eventually see themselves as part of a “panethnic” group (Enloe 1981; Espiritu 1992; Okamoto 2003). As these different individuals come together, they may develop common interpretations of their experiences (Cornell 1988), which could become the foundation of future political action (Blauner 1972; Espiritu 1992). Hence, racial discrimination may lead many members of the second generation in Spain to increasingly identify panethnically as “Asian,” “black,” or perhaps more generally as “people of color.” This could lead to future political mobilizations (Okamoto 2003), which could influence the ethnic logic of the Spanish state itself by demanding official recognition of ethnic-based cleavages and social policies that address inequality and discrimination (Wimmer 2008).

In this paper, I show that some immigrants will use the category of race to describe their discrimination experiences even in a country like Spain, where race is not formally available in the official ethnic repertoire. Further research is needed to understand where racial discourses come from. One possibility is that the diffusion of the category of “race” to talk about perceived group differences originates in supra-national organizations such as the European Union, which passed resolution 43/ED against unequal treatment on the basis of “racial or ethnic origin” in 2000.⁹ Other sources could be educational textbooks and the media (van Dijk 2005).

Some caveats are in order. While I find that very few immigrants report religious discrimination, religious-based opposition to immigrants in Spain could still be significant. For example, religious concerns could be a significant factor driving native attitudes and the political discourse of elites even if immigrants do not experience religious-based exclusion in their everyday lives. Indeed, though in this study I present evidence on the composition of ethnic boundaries in Spain from the point of view of immigrants, the nature of these boundaries also depends on natives’ conscious and subconscious biases. In addition, future research should assess whether perceptions of discrimination actually limit the successful integration of immigrants into Spanish society. Some nationality groups might report more discrimination precisely because they are attempting to break into the mainstream and become more upwardly mobile. Future scholarly work should also try to incorporate information on the actual appearance or phenotype of immigrants, as scholars are currently doing in other regions (Telles and Flores 2013; Massey and Sánchez 2010). This is an important concern given the significant racial diversity that exists in several immigrant-sending countries, particularly in Latin America (Telles and Flores 2013).

Those caveats aside, however, this paper provides strong evidence that, due to recent demographic changes, Spain is in the process of becoming a racially differentiated society featuring deep-seated race-based boundaries.

Notes

1. Great Britain is an exception from this trend in Europe. Not only does the British government recognize racial cleavages among the population, but also British social scientists have a long history of using this concept (see, e.g., Gillborn 1997; Heath, Rothson, and Kilpi 2008; Mac an Ghail 1989).
2. Due to the significant number of Catalan speakers in Barcelona, I also tried several coding schemes measuring native language skills as mastery of either Spanish or Catalan. All substantive results were robust to the use of either specification. In addition, I include an indicator for Barcelona residents in all regression models to account for region-based differences in language use.
3. While 87.7 percent of 1.5-generation Moroccans identify as Muslim, 68 percent of immigrants from the Indian subcontinent, which includes Pakistan, also identify as such in the ILSEG survey.
4. In these graphs, I grouped some nationalities that share some characteristics, such as Argentineans, Uruguayans, and Chileans grouped as "Southern-Cone," and Bolivians, Peruvians, and Ecuadoreans grouped as "Andean." I also grouped Chinese, Filipinos, and individuals from the Indian subcontinent into an "Asian" category and Romanians, Bulgarians, Polish, Russians, and Ukrainians as "Eastern European."
5. I thank Juan Díez Nicolás for generously sharing these data.
6. According to the study's documents, immigrants were approached in ethnic businesses, immigrant organizations, and other places of high concentrations of first-generation immigrants. Hence, the surveys are not representative. Nevertheless, they could still provide suggestive evidence on immigrants who can be located through these channels.
7. The question was "Have you had problems finding a job when you have searched for one?" The response categories were a few times, most times, always, never, and "hasn't searched for a job"; 72.41 percent of all job seekers reported "problems" and hence were asked the follow-up open-ended question on the nature of these problems.
8. These calculations were done adding the four waves of the surveys to increase sample size (2000, 2001, 2002, and 2004).
9. I thank an anonymous reviewer for this insight.

About the Author

René D. Flores is a Robert Wood Johnson Scholar at the University of Michigan and an Assistant Professor of Sociology at the University of Washington. His primary research interests are in the fields of international migration, race and ethnicity, and social stratification. In his past work, he has examined the social consequences of subnational restrictionist immigration policies in the United States. His work has appeared in *American Sociological Review* and *American Behavioral Scientist*.

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Negotiating Migration, Performing Gender

Anju Mary Paul, *Yale–NUS College*

Increasing numbers of independent women labor migrants leave countries in the Global South every year to work overseas. However, our understanding of how exactly gender and migration intersect at the decision-making moment is still inadequate. The new economics of labor migration (NELM) argument that individual migration is a household-level decision has been criticized by feminist scholars for ignoring the gendered social norms and inequitable intra-household power distribution that can make it difficult for prospective independent female labor migrants to leave their homes to work overseas. To reconcile NELM with gender reality, I propose an explicitly gendered, “negotiated migration model” that separates the pre-migratory process into three parts: an individual-level aspiration, the household-/family-level negotiation, and only then, the migration decision. The intermediate negotiation phase is a dynamic, two-sided, discursive site where *both* the aspiring migrant and her relatives engage in gendering practices and gender performances to bolster their respective positions. Interviews with 139 Filipino migrant domestic workers reveal that successful female migrants win their families’ support by coopting gendered scripts prevalent in Philippine society. Rather than attempting to “undo” gender, these women reframe their migration aspirations as a duty, rather than a right, to migrate, and a logical extension of their traditional, supporting roles as daughters, wives, sisters, and/or mothers. Thus, even though these women migrants break gender barriers when it comes to their independent labor migration, they do so by “doing,” rather than “undoing,” gender.

Increasing numbers of women are leaving their families in the Global South to work overseas as independent labor migrants¹ (Engle 2004; Ehrenreich and Hochschild 2002). The new economics of labor migration (NELM) theory argues that it is households² that dispatch these women to seek work overseas in order to expand and diversify the household’s joint income stream and reduce their

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relative poverty vis-à-vis other households in their reference group (Findley 1987; Katz and Stark 1986; Lauby and Stark 1988; Stark and Bloom 1985; Taylor 1987). But the classic NELM model of the unified family ignores the gendered social norms and inequitable intra-household power distribution that exist in patriarchal societies and can constrain the opportunities for independent migration among aspiring women migrants (Boyd and Grieco 2003; Chant and Radcliffe 1992; Curran et al. 2006; Folbre 1986; Grasmuck and Pessar 1991; Hondagneu-Sotelo 1994; Kana'iaupuni 2000; Radcliffe 1986; Wolf 1992; Oishi 2005). Newer economic models of intra-family decision-making recognize that “non-cooperative bargaining” often takes place in households (Lundberg and Pollak 1996, 2003). I build on this current understanding of family dynamics to argue that, before any individual migratory action is taken, a household-level migration *negotiation* has to occur over an individual family member’s migration aspiration.

Economic factors such as the relative wages, employment status, occupational status, job stability, and likelihood to remit, of individual members of the household determine their negotiating power during these intra-familial bargaining sessions (Lauby and Stark 1988; Lundberg and Pollak 1996, 2003; Mincer 1978). But there are also cultural conventions about the relative entitlement of different family members to pursue their own preferences and the worth of their relative contributions to the household economy that influence this bargaining process (Lundberg and Pollak 1996; Sen 1990; Cooke 2008). These cultural conventions can make it easier for men to migrate independently (Pfeiffer et al. 2008; Hoang 2011b; Hondagneu-Sotelo 1994) or, in the case of family migration, for couples to migrate to suit the husband’s preferences rather than the wife’s (Cooke 2008; Mincer 1978). The rise in women’s independent labor migration appears to go against these cultural conventions, and we do not have a clear conceptual understanding of how independent women migrants navigate these gendered norms about whether women have the “right” to migrate.

Qualitative feminist studies on the “patriarchal bargains” (Kandiyoti 1988) that women pioneers make in male-dominated spheres have much to contribute here (see Pini 2011; Yodanis 2000; Williams 1989). I draw on this scholarship to conceive of the intra-familial migration discussion as a dynamic, two-sided, discursive site where *both* the aspiring woman migrant and her relatives engage in “gendering practices” (Martin 2003) and gender performances to bolster their respective positions. As they consider whether the aspiring migrant should leave the home country, both sides are negotiating their understandings of appropriate gender roles within their household. But I also find that these women are “doing,” rather than “undoing,” gender (Butler 1988, 2004; West and Zimmerman 1987) by presenting their migration aspiration as a duty, rather than a right, to migrate.

In making this argument, I draw upon in-depth interviews I conducted with 139 Filipina migrant domestic workers (MDWs) in five countries between 2008 and 2011. These interviews revealed that aspiring female migrants had to negotiate gender norms that exist in Philippine society that cast them as the primary caregivers within their households, while their fathers/husbands were seen as the primary breadwinners. In response, my interviewees frequently framed their independent migration aspirations in gendered terms, presenting themselves as dutiful

daughters, caring mothers, and/or supportive wives, promising to remit most of their overseas earnings in order to ensure the future livelihood of the family members they left behind. They appropriated Philippine norms about a woman's role as primary care provider in the household and reframed their labor migration (and consequent separation from their families) as a way of *fulfilling* this role rather than violating it. These women's gender performativity highlights their constrained agency to win their family's support. Rather than seeking to completely upend the gender hierarchy within their families, they instead coopted prevailing gender discourses in order to win over powerful stakeholders. In this manner, women's independent labor migration—enabled by calling upon ideas of women as long-distance “helpers” and “nurturers” for their families—can paradoxically reify the existing gender order in sending countries.

Gender and Migration

Over the past 20 years, an increasing amount of scholarly attention has been paid to the independent labor migration of women. It is now well accepted that women migrants face different drivers and opportunity structures than men (see Boyd and Grieco 2003; Curran et al. 2006; Hondagneu-Sotelo 1994; Morrison, Schiff, and Sjöblom 2008; Pedraza 1991; Pfeiffer et al. 2008). Macro-level forces such as the growing demand for low-wage, pink-collar services in the Global North disproportionately facilitate the outmigration of female labor migrants from the Global South (Lan 2006; Parreñas 2001; Sassen 1988, 2006; Oishi 2005). On the sending country side, female brain drain from developing countries is 17 percent higher than male brain drain (Docquier, Lowell, and Marfouk 2009), possibly because of educated women's frustration with the social constraints and lack of career opportunities they experience in their home country (Kana'iaupuni 2000; Richter and Taylor 2008). At the meso-level, migrant networks have been found to operate in gendered ways, with male migrants facilitating the outmigration of other men, and female migrants helping other women (Curran et al. 2005; Curran and Rivero-Fuentes 2003; Hoang 2011a; Kana'iaupuni 2000). But our understanding of how exactly gender and migration intersect at the decision-making moment is still inadequate (Pfeiffer et al. 2008).

Independent labor migration can certainly hold much appeal to women. It is seen as a means of supplementing their personal/family income, and escaping unhappy/abusive marriages and familial/societal expectations (Grasmuck and Pessar 1991; Hondagneu-Sotelo 1994; Lan 2006; Parreñas 2001; Oishi 2005). However, living apart from their families often forces migrant women to renegotiate, rather than completely escape, normative ideas of femininity, motherhood, and domesticity (Hofmann and Buckley 2011; Lan 2006; Parreñas 2001). Women have to find new ways to fulfill their roles as wives, mothers, and daughters when they are located in a different country/continent from their family (Parreñas 2001). Pei-Chia Lan discusses how the feminization of overseas domestic work has led to the emancipation of Filipina and Indonesian women but also served to sustain notions of “gender subordination” (2006, 126) because their overseas work reifies traditional notions of the woman as caregiver and nurturer. These scholars focus on the patriarchal bargaining that migrant women have to engage

in when they are already working overseas. In contrast, I look at the bargaining that begins even before any migration has occurred, when it is still only an idea that is being discussed within the aspiring migrant's household/family.

In outlining the nature of the intra-household bargaining that aspiring women migrants undertake, I draw on studies of opposite-sex pioneers in heavily gendered work sectors. Women undertaking gender-atypical work often justify their incursion into male territory by framing it as a natural follow-on from their existing roles as mothers or wives. See Pini (2011) about women's farm networks, Williams (1989) about female military officers, and Yodanis (2000) about fishing women. Meanwhile, men entering female-dominated fields (such as nursing or music teaching) adopt masculinizing discourses in their at-work manner and attire in order to de-emphasize the feminized reputation associated with their new job (Roulston and Mills 2000). These men and women are not performing their gender in a vacuum. Rather, as social actors, they are engaging in dialogue with existing, hegemonic societal narratives about appropriate gender roles and also with resistant/disapproving discourses they may encounter at an interpersonal level. In all cases, however, these female and male pioneers opt to "do" their ascribed gender, rather than "undo" it to justify their incursion into a gender-atypical occupational field. This literature informs my analysis of my interviewees' gender performances when negotiating with family members over their migration aspirations. But first, I outline what the existing literature on household-level bargaining has to say about the migration decision.

Households and Migration

The New Economics of Labor Migration (NELM) emphasizes the role of households/families in the migration decision, viewing the household unit, rather than the individual migrant, as the primary migration decision-maker. NELM theorists have proposed several household decision-making models.

The "Unified Household" Approach

The intra-household decision-making model most associated with NELM is that of a "unified household" reaching a common consensus as to which family member to send overseas. This model assumes a moral economy (or a joint utility function and common preferences) existing across the family (Lacroix 2010; Stark and Levhari 1982). Feminist migration scholars have routinely criticized this approach for ignoring the unequal distribution of power between men and women in patriarchal households and the ideological constraints on women's independence that can severely limit potential women migrants' ability to garner support from family members for their migration aspirations. Studies of women migrants from contemporary rural Brazil, rural Mexico, Malaysia, Indonesia, rural Bangladesh, Vietnam, and the Philippines have found that these women encountered varying degrees of pushback from parents, husbands, and fiancés when they first announced their desire to migrate (whether overseas or to urban areas within their own country) in order to seek employment (Afsar 2003; Aguiar 1975; Arizpe and Aranda 1981; Hoang 2011b; Hondagneu-Sotelo 1994;

Lan 2006; Stichter 1990; Wong and Ko 1984). In some cases, these young women had to delay their migrations until their parents relented and permitted them to leave home (Aguilar 1975). In other cases, parents vetoed certain destinations as being too dangerous, too licentious, or simply too far away (Arizpe and Aranda 1981). Husbands can also resist their wives' independent labor migration (Gamburd 2000; Hoang 2011b; Lan 2006).

The "Household Dictator" Approach

The "dictator" model takes as given that power hierarchies along gendered and generational lines exist within most families. This model assumes an all-powerful head-of-household who commands complete obedience from and makes migration decisions on behalf of the entire family. Such a despot might decide to send a family member overseas because he—and the head-of-household is usually a "he"—can force that individual to send remittances back to the family (Lauby and Stark 1988). Or he sends away a family member whom he sees as potential competition (Grasmuck and Pessar 1991).

This view of household decision-making explains the resistance many aspiring women migrants encounter. But some NELM theorists use this model to argue that independent women labor migrants may be consciously dispatched by their household heads. Stark, Micevska, and Mycielski (2009) find that the eventual household member to go abroad is usually the one who stands to earn the most from working overseas and is therefore potentially able to remit the most money to the remaining members of the household. Following this line of thinking, if a gendered overseas labor market exists that provides more lucrative job opportunities for women migrants, a household head might decide to send a female relative—a wife or daughter—overseas. Households might also send forth their daughters, rather than sons, if the former are expected to remit a greater proportion of their earnings (Lauby and Stark 1988). But both sets of authors draw their conclusions using only survey data about migration flows and remittance levels without directly investigating the internal family dynamics they hypothesize about. The individual agency of these female independent migrants is also not taken into account, and it is assumed that families are completely swayed by economic reasoning, without any influence from socio-cultural norms.

The "Super-Trader Family" Model

The third model of family decision-making recognizes at least two autonomous decision-making entities within the household—the potential migrant and other relatives—each with their own distinct preferences and differentiated capacity to act (Stark and Bloom 1985). Model proponents argue that, before any migration occurs, the potential migrant and her family enter into an agreement involving "intrafamilial trade in risks, coinsurance arrangements, devices to handle principal agent problems, moral hazard problems [...] and contract enforcement problems [...] and, overall, striking a mutually beneficial, intertemporal, self-enforcing contractual arrangement" (Katz and Stark 1986, 136). Amartya Sen has satirized this model as the "super-trader family" but, at one level, this view of

household decision-making makes intuitive sense because it recognizes both an agentic individual migrant and the existence of multiple decision-makers within a household.

Economists had initially assumed that this intra-household bargaining was cooperative, which would ensure that families would always choose the most “efficient” outcome in their intra-household decision-making about the allocation of resources, despite holding different preferences (Manser and Brown 1980). If this were the case, then irrespective of which family member was offered a better-paying job in another location, the family would make the moving decision that brought the highest net utility to the household as a whole. However, empirical data on US family migration shows that non-egalitarian families typically do not move if it is the wife, as opposed to the husband, who is offered a better job elsewhere (Cooke 2008).

Today, economists accept that *non-cooperative* bargaining, influenced by non-economic conventions, often takes place within households, resulting in families making “inefficient” decisions (Lundberg and Pollak 1996, 2003; Sen 1983, 1990). Different family members’ perceptions about the proper role of women in the intra- and extra-household division of labor, the kinds of contributions women should make to the household economy, and the amount of benefits (and rights) women are entitled to, all influence intra-familial negotiations on a range of topics. Even if it is in a family’s financial best interests to send one of their womenfolk overseas to work, other family members may still not support such a move if it runs counter to their views about a woman’s proper role in the household.

Several studies have highlighted the strategies adopted by aspiring women migrants to navigate around this familial resistance (see Hondagneu-Sotelo 1994; Lan 2006; Oishi 2005), but Lan Anh Hoang’s (2011b) work on male and female internal migration in Vietnam comes closest to recognizing this process as an intra-familial negotiation. Hoang identifies four patterns of household-level decision-making among her sample—uncontested, consensual, negotiated, and conflictual³—but classifies the vast majority (81 percent) of her migrant interviewees as having undergone a “consensual” decision-making process with their family members, where “household consensus was obtained prior to migration” (1446), while a mere 3 percent of her respondents reported having “negotiated” with household members to “resolve [any] conflict of preferences” (1446) prior to migration. Little detail is provided to clarify this demarcation between consensual and negotiated decision-making, and I take issue with how Hoang categorizes the household decision-making processes that her interviewees described to her. This categorization appears to contradict her own account of the gendered ideas that pervade Vietnamese rural society about women’s normative role as primary caregivers within their families. Hoang writes that the women migrants she interviewed “almost always made their migration decisions in consultation with families, particularly with the husband if they were married—and with parents if they were not” (1450). If aspiring migrant women adopt gendered strategies to neutralize any potential resistance from relatives, even *before* it manifests itself, how should this “consultation” be classified? As Hoang herself notes, married female migrants believe familial consensus to be “essential” (2011b, 1450), and so

they may have tried to present their migration aspiration to family members in a manner so as to curtail any potential resistance. Meanwhile, Hoang's male migrants—almost all of whom are categorized as having undertaken “consensual” household-level decision-making—relied on gender norms about their role as family protectors and breadwinners to such an extent that their wives did not voice any concerns/complaints about their migration even though these women were unhappy about their husbands' departure (1451). Again, I do not think such cases should be classified as “consensual” household decisions.

Instead, I argue that *all* intra-household migration decision-making is negotiated, with variations in the degree of overt resistance/support posed by family members. Aspiring migrants may not be in explicit conversation with family members about their migration preferences and roles, but they are continuously grappling with the gendered norms that pervade their society that either support or constrain their mobility.

Studying Filipino Migrants

The data used in this article are drawn from a larger study investigating destination decision-making among international labor migrants (Paul 2011, 2013). Within the global population of international migrants, I chose to focus on *Filipino* migrants because the Philippines is a major supplier of temporary labor migrants in a range of occupations for the world market. More than 1.8 million Filipinos left their home country in 2012 as temporary migrant workers (POEA 2013). Among land-based Filipino migrant workers, domestic workers form the single largest occupational category, with more than 95 percent of them being women (POEA 2010). Filipino MDWs can be found in over 100 countries and territories around the world. I designed the study to compare the destination decision-making processes and strategies of Filipino MDWs in four popular destinations: Singapore, Hong Kong, the United States, and Canada.

Pressure on Filipinos to seek work abroad comes from the high rate of *under*-employment in the country, reaching 19.5 percent of the employed in January 2014,⁴ and the high individual poverty rate of 24.9 percent in 2013.⁵ This is especially true for Filipino women, who are typically employed in lower-paid, lower-status positions. The female “vulnerable employment” rate was 46.1 percent in 2009.⁶ Women's careers are also considered less important (Medina 2001), which can make it easier for women to jettison their local jobs and seek work overseas. Pei-Chia Lan (2006) writes that the steady decline in real wages has made overseas employment more tempting, even for middle-class families. For Philippine families with college-age children, the failure of credit markets to make tertiary education more affordable (Kitaev et al. 2003) may also propel one or both parents to consider overseas work.

During the 1970s and 1980s, the Philippine government actively promoted the outmigration of Filipinos. Labor migrants were socially constructed as national heroes and martyrs, sacrificing their personal comfort for the sake of their families and the nation (Rodriguez 2010; Tyner 2004). More recently, this labor migration policy has been called into question as grassroots organizations have highlighted the toll that women's labor migration has taken on Philippine families (Rodriguez 2010).

There are other countervailing forces that can inhibit Filipino women's ability to seek work abroad. The Philippines is a patriarchal society, with men seen as the natural heads of households (Chant and McIlwaine 1995; Eder 2006; Eviota 1992; Roces 2000). Even as more Filipinas take on formal employment outside the home, their intra-household status remains second to that of their menfolk, and they are still expected to be the primary care providers at home. Philippine husbands have more authority to override joint household decisions they had previously made with their wives, and Filipina wives often give in to their husbands' wishes for the sake of family harmony (Medina 2001). Filipino daughters (and sons) are also very filial, demonstrating great respect for and deference to their parents' wishes (Chant and McIlwaine 1995; King and Domingo 1986; Medina 2001; Trager 1988). But before presenting how my interviewees' families responded to their migration aspirations, I first discuss how I collected my data.

Data and Methods

Between 2008 and 2011, I conducted in-depth, semi-structured interviews with 139 Filipinos in the Philippines, Hong Kong, Singapore, and Canada about their decision to work overseas as temporary domestic workers.⁷ In each study site, participants were recruited by directly approaching Filipino migrant workers in public places that were popular weekend gathering spots for these migrants and by snowball sampling. I also posted flyers advertising the study in these weekend gathering places, in the offices of local NGOs that work on MDW issues, and at maid recruitment and placement agencies. Given that I was studying destination decision-making, the sample was constructed to allow comparisons between the experiences of migrants who had traveled to different countries. As such, the sample did not allow for a comparison between male and female migrants (as most Filipino MDWs are women), or between voluntary migrants and involuntary non-migrants. The implications of these limitations are discussed later.

During recruitment, I introduced myself as a student researcher from the United States studying the migration and destination decisions of Filipino MDWs. Participation was restricted to Philippine nationals, 18 years old and above, who were former, current, or prospective overseas domestic workers. Even though participation was not restricted on the basis of gender, all but two of my interviewees were women, given the heavily gendered nature of paid domestic service. All interviews were conducted in English, with only two participants requiring an interpreter. All names have been changed. Table I provides some descriptive data about the interviewees.

A significant portion of each interview was spent discussing the mechanics of migrants' initial migration decision-making process. In all country sites, I asked each respondent: "How did you come to decide to leave the Philippines and work overseas? Did anyone express reservations about this idea?" If the migrant answered "yes" to the latter question, I asked for details and asked how she had managed to overcome this resistance.

Table I. Characteristics of Sample, by Country

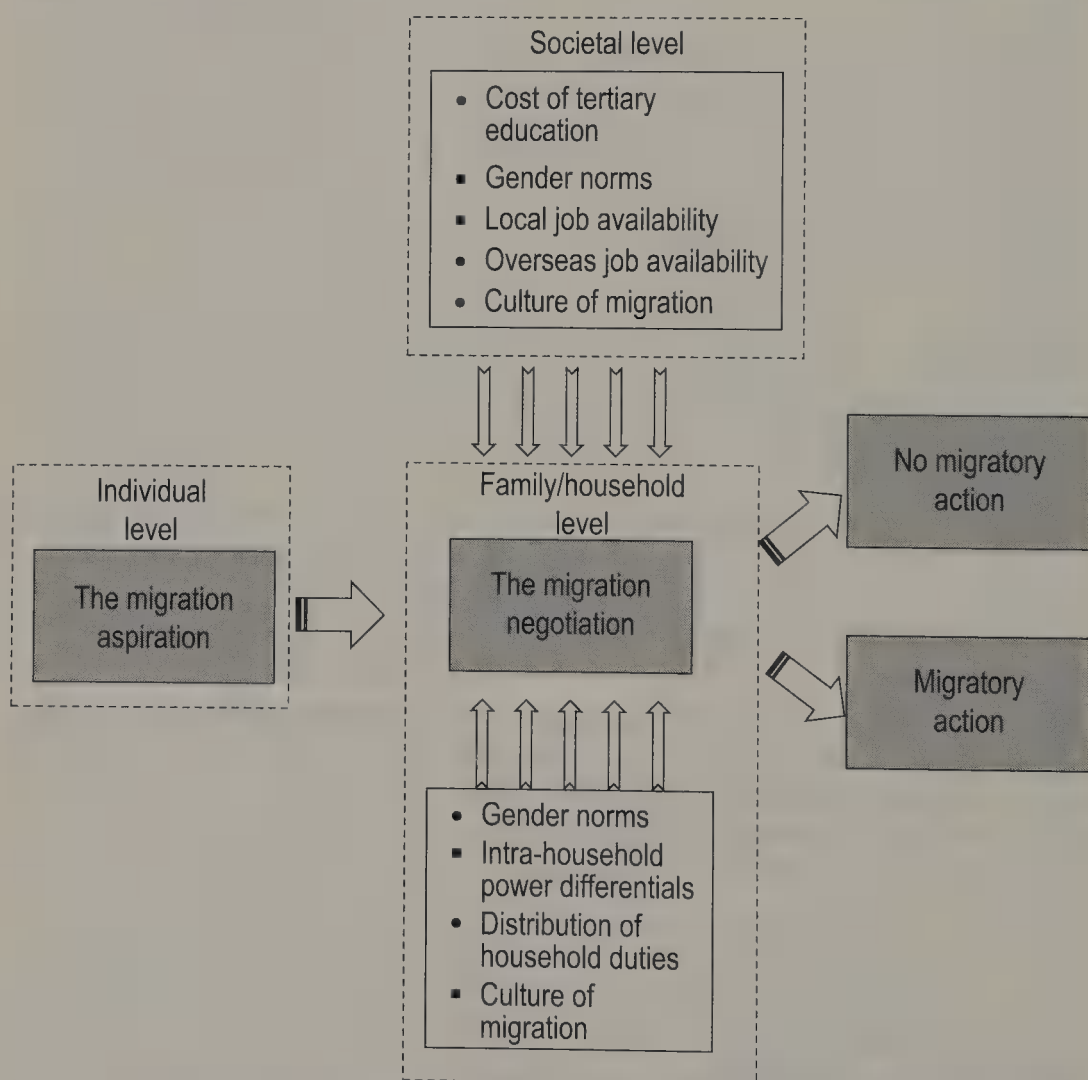
	Hong Kong (<i>n</i> = 28)	Singapore (<i>n</i> = 41)	Philippines (<i>n</i> = 26)	Canada (<i>n</i> = 44)
Migrant characteristics	Percentage of population			
Age (in years):				
18–25	—	5%	23%	5%
26–35	43%	41%	15%	45%
36–45	36%	34%	38%	27%
46 and above	21%	20%	23%	23%
Gender:				
Male	7%	—	—	—
Female	93%	100%	100%	100%
Marital status:				
Never married	50%	32%	35%	35%
Married	46%	54%	35%	43%
Separated	4%	10%	15%	21%
Widowed	—	5%	15%	—
Education:				
Less than high school	11%	5%	8%	—
High school	18%	29%	38%	—
Some college	36%	41%	35%	22%
College and above	36%	24%	19%	78%
Departure from Philippines:				
1970s	7%	—	—	5%
1980s	14%	17%	19%	5%
1990s	36%	41%	23%	14%
2000s	43%	41%	58%	77%

While I was familiar with the ongoing debates over NELM, I had no starting hypothesis about what actually happened within female migrants' families prior to their migration. Instead, I adopted an inductive, discovery-driven approach (Burawoy 1998; Glaser and Strauss 1967; Kleining and Witt 2001). It was only during the coding of the interview transcripts that I detected a pattern of two-sided, intra-household negotiations over gender norms taking place. Phrases such as "I said, 'I am doing this for you,'" "for the family," and "I want to help" made regular appearances in the transcripts as interviewees recalled how they presented their migration aspirations to relatives. These interview transcripts were then coded using both closed and open coding techniques for any mention of familial resistance (or lack thereof), and different approaches for overcoming said resistance.

Findings: Negotiating Migration

My interviews made clear that independent labor migrants' migration decisions almost always started out as an individual-level idea that was then presented to various family members for consideration and approval. Only one of my interviewees spoke of having jointly considered the idea of labor migration with her husband from the very start. All others spoke of first concluding that they wanted/needed to work overseas—often through the influence of network contacts who were already overseas MDWs—and only then broaching the topic with their immediate family to gain their relatives' support. (Figure 1 provides a pictorial representation of the three-step pre-migratory process of (1) aspiring, (2) negotiating, and then (3) decision-making that most of my interviewees went through.) I posit that the intensity of the negotiation phase of this pre-migratory process is dependent on the gendered context (at both the household and the societal level)

Figure 1. A negotiated migration model



Note: This model ignores resource and policy considerations that also determine whether any migratory action will take place. It also sets aside the social forces that lead individual migrants to consider independent labor migration in the first place.

in which the aspiring migrant lives, setting aside resource and policy considerations. In situations where there are well-accepted norms of female independent migration, reduced patriarchy, and a skewed overseas labor market that privileges female workers, the negotiation process should be less fraught for aspiring migrant women. Likewise, aspiring male migrants should face limited resistance to their migration dreams in societies marked by high levels of patriarchy and entrenched tropes about the male provider (Hoang 2011b). It is these familial and societal gendered norms, more so than individual relatives, that all aspiring migrants negotiate.

My hypothesis is supported by the fact that only 55 percent of my interviewees recalled encountering *overt* resistance from one or more of their immediate relatives: their parents, husbands, older siblings, or children.⁸ Over half of these interviewees encountered overt resistance to their migration aspirations from their parents, either both or just one parent. Among married participants, 49 percent faced resistance from their husbands. These relatives' overt resistance manifested itself in various ways, including a refusal to cover the costs of recruitment agency fees and travel costs, a refusal to care for the aspiring migrant's children during the time period she wanted to be overseas, the barring of daughters from leaving the country, and the withholding of verbal support and approval.

Even in cases where female migrants did not face *overt* resistance, women spoke of "asking permission" from parents and/or husbands before going abroad. Their choice of words demonstrates the unequal power structures (along generational and gendered lines) that prevail in Philippine households and how aspiring women migrants needed to prepare for *potential* resistance to their migration aspirations. For young, unmarried women, household authority was vested in their parents, while for married women, their husbands were the heads of the household.⁹ But married women migrants still often sought their parents' blessings in addition to their husbands', partly because they craved the emotional reassurance of knowing their parents supported their decision, but also because they often relied on their parents to help look after their children while they were away (see also Hoang 2011b).

Parents who expressed explicit resistance to their daughters' migration aspirations justified their position mostly by raising concerns about their daughters' safety, regardless of how old these women were at the time. The isolation their daughters would experience overseas was another common concern. Desiree, who had left the Philippines when she was 25, recalled how her father told her that "it is too far away and then you are alone." Ariel in Singapore remembered her mother telling her, "No, you just work here [in the Philippines] because, when you go to other country, you don't know what might happen to you." Parents (such as Matilda's father quoted below) were also more likely to directly raise their daughters' normative role in the household as justification for withholding support:

My father doesn't want me to go abroad. "Because," he said, "it's not you to go anywhere. It should be your husband. He is the man. He should be the one to give you everything you need."

Likewise, Aisha's father had no issues with her brother working overseas but balked when Aisha first raised the notion of working in Brunei as a domestic worker:

Of course, I'm not the first one in the family to go [overseas]. My brother is in, at that time, Saudi. But he's a guy, so [my father] won't mind. But I am a girl, so it's a big issue.

Several interviewees echoed Aisha's matter-of-fact statement about her father's double standards. It was taken for granted that parents would worry more about the safety of their daughters, and that parents should have more say in their daughters' migration decisions.

Husbands, meanwhile, often argued that their family's existing standard of living in the Philippines was already comfortable and did not require raising. Other migration scholars have also noted husbands' resistance to their wives' potential new status as family breadwinners and the change in their own status to that of "househusband" (Gamburd 2000; Lan 2006; Parreñas 2001; Pignol 2001; Hoang 2011b). Christine, who worked first in Hong Kong and then Canada, recalled how her husband said there was no need for her to work overseas:

But, for my husband, at first, he really don't want me to leave. He said, "We have our daughter and I can still provide you our needs." But I said, "Yeah, you can provide me the needs right now. But how about if we have more children? If I give birth again?" I said that for the future of our family and the future of our children, in case we are going to add more, then I really need to go. I really need to go abroad.

For women with children, relatives used explicitly gendering discourses about interviewees' roles as mothers to try to convince them not to leave. Marnie, a domestic worker in Singapore whose four children were between 10 months and seven years in age when she left the Philippines, encountered such resistance from her mother. Marnie's husband was a farmer in the Philippines whose earnings were minimal at best, but Marnie recalled her mother telling her "that 'I can't do anything' because I have my own family already." Lena, who had also been a housewife in the Philippines before she left for Hong Kong at the age of 25, spoke of her parents telling her that "it is very hard to leave [your] child with another," implying that her husband or her parents would not be adequate substitutes for Lena's own presence. Linda, a domestic worker in Singapore, told me how her relatives criticized her for abandoning her children, who were all girls. "They say that it is hard to leave them. 'Who can teach them? Who can guide them? That is what a mother should do,'" Linda recalled them telling her. All in all, women migrants' relatives adopted gendering discourses to emphasize women's primary nurturing roles in the home, their vulnerability overseas, and their secondary status in relation to their brothers, husbands, or fathers, in order to bolster the argument that these women should not be allowed to work overseas.

Findings: Doing Gender

In response to this overt familial resistance or to neutralize any potential resistance, aspiring women migrants adopted several strategies. These involved a very

particular performance of gender in these women's discursive presentation of their aspiring migrant self to relatives. Interviewees emphasized their gendered identities as dutiful daughters, caring mothers, and/or supportive spouses, framing their desire to work overseas as an exclusive act of intra-familial loyalty and self-sacrifice. They highlighted how labor migration would allow them to *fulfill* their roles as mothers, daughters, and sisters through the greater income they could make available to their families. They also emphasized the negative consequences of inaction, in terms that made it clear that *not* migrating would make them less-than-adequate mothers, sisters, or wives. What they never mentioned to resistant family members were the more self-directed motives behind their migration aspirations, such as their desire to see the world, be independent, live in the West, escape a failing relationship, or jumpstart their careers. (These more self-directed motivations were, however, mentioned to me). They also did not argue that they had a right to migrate.

The migrant as dutiful daughter

In trying to convince their parents to support their migration aspiration, unmarried interviewees often framed their reason for migrating as a desire to meet family-based needs. Twenty-four of my unmarried interviewees adopted this "dutiful daughter" approach. In so doing, aspiring migrants were assisted by the Philippine tradition of never-married women taking on the responsibility of caring for their parents and supporting their younger siblings' education (Chant and McIlwaine 1995; Lan 2006; Lauby and Stark 1988; Medina 2001).

Desiree pointed out to her reluctant father that working overseas would allow her to fund her siblings' university educations, something that her father was unable to do:

He doesn't like me to go here. He said it is too far away and then you are alone. And so I said, "If you want help to finish all my brothers' and sisters' education, let me go." Because my two brothers, the two younger brothers, they are so clever. They say they want to learn, to study.

In this manner, Desiree couched her migration aspiration in the accepted language of familial duty, presenting herself as the filial daughter who would be helping her father fulfill his responsibilities as household head. Likewise, 44-year-old Aisha, who left the Philippines for Brunei at the age of 28, explained that her father eventually agreed to support her labor migration only after she convinced him that she could help raise the entire family's living standards:

I say ... at least, I can help out the family. And I say, we can raise up the living condition. And I tell him, "Didn't you want to have our own house? Buy a small land?" And he smiled bitterly. But I know. He's a father. Then, after that, he said okay.

Another migrant, Rory, who was about to commence her second contract in Saudi Arabia when I interviewed her, recalled how she won over her parents in a similar manner:

My father ... does not like I go to other country because he has heard about, through the television, about accidents. ... But, for me, I am decided. So I told my father, "I want to go, I want to help all my family."

Interviewer: Your mother? Was she okay with you leaving?

No, at first, it's not okay. She is not okay. But after that, I ask my mother, "I want to go, I want to go. Not only for myself. It is just only for you." [And] after two years [overseas], I have no money now. [...] My money, all my salary in two years, I give it to my family. I give all to my family.

The migrant as caring mother

The criticism typically leveled at aspiring migrants with children was that they were being irresponsible mothers, abandoning their children to go gallivanting overseas. Interviewees tended to respond by emphasizing how working overseas (on a temporary basis) was in fact the exact opposite of irresponsibility, but rather the best way for them to *fulfill* their role as mothers. Thirty-four of my interviewees who had young children when they first left the Philippines adopted this strategy.

Linda, the domestic worker in Singapore with three daughters, emphasized how it was her concern for her daughters' education and day-to-day standard of living that compelled her to seek work overseas. Diane, a married mother of two, recalled how she had to convince her husband that her overseas domestic work was the only way for them to provide for their children:

My husband is very reluctant for me to leave. But I said, "If I stay here [in the Philippines], what will happen?" It's not that we will go hungry. We won't become like that. But we have got two kids and the kids are growing. So I said, "If we stay with this income, more likely the future of the kids will be a question mark. So maybe if I go there, maybe I can give our children a brighter future."

Diane did not want her husband to think she saw him as an inadequate provider. At the same time, she emphasized how the financial cost of a tertiary education in the Philippines was too burdensome for a single local income—even *two* local incomes—to cover. A similar situation occurred with Millie, a trained midwife who first worked in Singapore for two years, then returned to the Philippines and sought to go overseas again to Hong Kong but was initially not allowed to do so by her husband:

After I went back home, my husband doesn't allow me to work [overseas] anymore. Yeah. So I look after my children for two years. But, because of the economy, I really need to come abroad and to look for work again.

Interviewer: How did you convince him to let you go again?

I just talked to him nicely. "You know, life here is so difficult and we want that the children, after growing up, to go to college or universities. But if

I'm just here to look at you, look at each other's face, nothing will happen for us. And then our children won't go to—we cannot send them to school, you know.” So he think it over and let me come again.

The migrant ■■ supportive wife

In the account above, Millie's choice of words—about talking to her husband “nicely”—highlights how married women had to tread lightly when negotiating prevalent gender norms about the male breadwinner. In such cases, interviewees framed their desire to work overseas as a common-sense response to the gendered overseas labor market that was geared toward *Filipino women* rather than Filipino men. Sometimes interviewees (like Matilda, below) had to use this argument with their parents rather than their husbands:

I told my father that I think I need to work because [my husband] is not required for any job going abroad. He is not skilled. That's why I preferred to just go abroad and then he [my husband] will be the one to look after my children.

Several other married migrants, like Janelle (below), presented similar arguments, emphasizing that they had no ambition to usurp their husbands' head-of-household role:

For a man, there's not really a lot of opportunity that time. It was always the woman who was faster to, you know, to leave the country. [So] I was only the one [to apply]. I said we cannot do both, because of the financial, you know? The financial cost of applying [for an overseas job]. If I will go faster, then just I will try. Because, you know, if I go faster, then I could start to earn [faster].

In such cases, women migrants were able to win their husbands' support by emphasizing that they were just trying to *help*. Twenty-four of my married interviewees used this kind of language. As Amarilla, in Singapore, put it: “I talk to my husband that I want to help him. First time, he do not want [me to go]. But maybe he realized that he needs my help. That's why he allowed me to work here in Singapore.”

These married migrants stressed that so-called “neutral” market forces, rather than any personal preference on their part, were dictating which spouse should go overseas. In this way, both wives (and husbands) could argue that the decision to send the wife overseas was simply a reaction to social forces that favored a woman's departure from the Philippines over a man's. Couching the migration decision in these terms made it clear that it was not that husbands were blasé about their children's future or that wives were feminist pioneers. Rather, the decision to let the wife work overseas was the most pragmatic option available to a family and simply a way for a wife to *support* her husband in his role as family head.

Other Tactics

A smaller subset of interviewees described adopting tactics that were not explicitly gendered. Among younger, unmarried interviewees, one popular tactic was to

avoid any mention of their migration plans until all the arrangements had been made (see also Oishi 2005, 105). This tactic worked for aspiring migrants who possessed sufficient funds to cover the cost of their migration themselves without having to rely on relatives. Interviewees who used this approach fixed their departure date and then, a week or two before leaving, announced their decision to their family. Jessica, who had worked in Manila before finding a job in Hong Kong, asked her parents (who lived on another island in the Philippine archipelago) to visit her in Manila a few weeks before her departure and told them then of her plans to go overseas. "If that's your decision, we cannot stop you," she recalled them telling her, despite being upset by the news. "Because you have already your visa, you have your tickets, and you are ready to go."

Eight other participants invoked the role of "fate," "luck," or "God" in shaping their future, making the argument that if they were destined to encounter difficulties, it would happen to them wherever they were. By placing their future in the hands of some higher authority, they were trying to alleviate any sense of personal responsibility or gendered concern their relatives might have felt.

Taking an opposite tack, other interviewees gave relatives a say over select parameters of their labor migration. These women spoke of changing their planned destinations because family members vetoed particular countries or regions as being too risky, delaying their departure for several years, or promising to return to the Philippines after only two years abroad. Still, most interviewees used these tactics in tandem with specifically *gendered* strategies in order to convince their relatives. For instance, Millie, the MDW mentioned earlier who talked "nicely" with her husband, also allowed him to veto any jobs in the Middle East, as he did not want her working for an Arab employer.

Discussion

The negotiations between aspiring women migrants and their families described in this article were almost always gendered and gendering in the scripts being called forth by both sides. The household discussion over labor migration was thus, at its heart, not simply a conversation over money—where to earn it, who should earn it, what it should be used for—but also a debate over gender expectations and roles. Families framed their resistance using normative beliefs that still enjoy wide circulation within the Philippines about a woman being primarily responsible for all social reproductive functions within her household and about her subordinate status vis-à-vis the male head of household and purported breadwinner of the family. Aspiring women migrants also called upon prevailing gender ideology in the Philippines but reworked it to support their migration dreams. They did make an economic argument à la NELM, pointing out the additional revenue stream they could provide their households and/or the straitened economic circumstances their families would have to endure if they did not work overseas. But these economic arguments only helped justify a household-level migration decision to send someone—anyone—from the family overseas. Migrants' gendered self-presentations as dutiful daughters, supportive spouses, and caring mothers were necessary to justify why *they* should be the family representatives to be sent overseas.

These intra-familial bargaining sessions do not match the unified household or dictatorship models of household decision-making, given that household authority figures—whether parents or husbands—had to be convinced that independent female labor migration was appropriate. The gendered and gendering negotiations that migrants engaged in do not match the third “super-trader family” model either. There may have been some bargaining over the amount of remittances to be sent back home each month or the allocation of these funds across various family members but, in essence, aspiring migrants were bargaining with the system of patriarchy that existed within their households and societies. During these negotiations, most interviewees “did” gender rather than attempting to “undo” it (West and Zimmerman 1987). They framed their migration as a way to “help” their families, rather than “provide” for or “protect” them. The former term is gendered as female, while the latter terms are male (de Beauvoir 1989[1949]; Smith 1987; Martin 2003). These women did not attempt to claim the mantle of family breadwinner in their migration negotiations even though that was what they would effectively become once they were overseas.

Conclusion

The concept of a *negotiated migration decision* that distinguishes between the individual-level aspiration to migrate and the household-level negotiation that needs to be successful before any migratory action can occur helps extend migration theory and align it with gender reality. This model of a negotiated migration decision parallels Everett Lee’s (1966) classic push-pull model of migration, which highlights that, between the migration aspiration and migratory action, there can exist “intervening obstacles” that prevent a desired migration from taking place. Lee highlighted the financial cost of migration, the distance between origin and destination, and the presence of dependent children as examples of intervening obstacles. More recently, Jørgen Carling has highlighted the role of capital and policy constraints in creating what he calls “involuntary non-migrants” (2002). My negotiated migration model focuses exclusively on the gendered cultural constraints on women’s independent labor migration aspirations. But it also squarely places a degree of agency, albeit constrained, back in the hands of these aspiring migrants as they attempt to navigate around these obstacles.

That is the second contribution of this article: conceiving of these intra-familial negotiations as dynamic, two-sided, discursive sites where *both* migrants and their relatives engage in gendering practices and gender performances. Given the gendered, socio-cultural constraints on their freedom of choice and movement, aspiring women migrants agentically reframe their migration aspiration as a logical extension of their supporting roles as daughter, wife, sister, and/or mother, rather than attempting to completely upend the gendered and generational power structure in which they are embedded. I am not the first migration scholar to note how independent women labor migrants from developing countries adopt gendered negotiating strategies with their families (see Hondagneu-Sotelo 1994; Hoang 2011b). But what my article does is take these empirical findings and incorporate them into a conceptual framework that explains the underlying processes at work.

However, the absence of interviews with involuntary female non-migrants makes it hard to ascertain what makes for a *successful* negotiation from the point of view of aspiring migrants. It is possible that involuntary non-migrants presented gendered arguments when attempting to leave the country, but that for some reason they were unable to persuade their relatives to support their migration aspiration. A follow-up study is being planned that will involve interviews with both involuntary non-migrants and voluntary migrants to contrast their respective negotiation experiences with their families in order to identify the factors that contribute to successful outcomes for aspiring women migrants. This study will also incorporate supporting interviews with other household members to verify migrants' accounts of their negotiation experiences and address concerns regarding retrospective interviewing as a research method. Still, the present study is useful in clarifying how successful migrants employ gender performance to frame and supplement the economic arguments they make for migration.

Other readers might take the opposite tack and argue that familial resistance would not have made any material difference to my interviewees' decision to migrate and that these women would have left the Philippines regardless. This would be a misreading of gendered family dynamics in the Philippines and elsewhere (see Aguiar 1975; Arizpe and Aranda 1981; Hoang 2011b). Several of my interviewees recalled how their first attempts to leave the Philippines failed because of stiff resistance from assorted family members. These interviewees used strong language such as "my parents did not *let* me go" or "my husband did not *allow* me to leave," indicating the coercive power these relatives had over their migration decision. This was not always a direct veto power; sometimes it was more akin to ensuring compliance by expressing disapproval. A couple of interviewees did leave the Philippines without informing their family members beforehand, driven by a fear that their parents would not support their migration decision. These women then called their parents a few weeks after they started their jobs to explain what they had done. But deciding to run away was not common among my sample. Instead, most interviewees tried to win their family's support before leaving. As Hoang (2011b) also notes with her Vietnamese women interviewees, it was important for my Filipina interviewees to feel that they had the backing of their husbands or parents in order to maintain their peace of mind while overseas by themselves, or because they needed these relatives to look after the children they left behind.

A final question remains: What about aspiring *male* migrants? Does the negotiated migration model apply to men? I say it does. Thinking of the intra-household migration discussion as a negotiation over gender norms/roles explains the greater ease with which aspiring male migrants are able to move, setting aside capital and policy considerations (Cooke 2008; Hoang 2011b). From a young age, men are traditionally granted much greater autonomy and freedom of movement by their families and society at large. In several cultures, the outmigration of young males from their villages is seen as a rite of passage and male non-migrants are publicly shamed for not undertaking this journey (Jónsson 2008; Lan 2006). Aspiring male migrants are also able to call upon well-established gendered tropes—such as the "male breadwinner," the "authoritative male" who does not need to ask for permission from others, and the "risk-taking

adventurer”—from their “cultural toolkit” (Swidler 1986) to stave off potential resistance. These presentations of their gendered selves to relatives help aspiring male migrants avoid possible overt resistance from their family (Hondagneu-Sotelo 1994; Hoang 2011b).

By deploying these gendering narratives with their families, male and female migrants inadvertently bolster conservative views about appropriate gender roles within a family. Even as women migrants carve out a figurative space for themselves as independent wage earners overseas, they cement the traditional view of Filipina women as self-sacrificial, self-effacing, and always putting their family’s needs before their own. This bind is apparent in Rory’s lament (mentioned earlier) that “my money, all my salary in two years, I give it to my family. I give all to my family.” Other interviewees spoke of the constant, never-ending demands for remittances from family members who took for granted that these migrants would send them money to meet all their needs all the time. Other scholars of Filipina migration have also observed these migrants’ practice of remitting practically all their earnings back to the Philippines (Lan 2006; Oishi 2005; Parreñas 2001). Interviewees spoke of how they were constantly inundated with demands from family members in the Philippines for more money. I posit that their sense of responsibility to send regular remittances was compounded by the Faustian/patriarchal bargain that these Filipina migrants had undertaken in order to win their family’s support in the first place. In this manner, interviewees’ pre-migration arguments paralleled the model of “Asian feminism” that is promoted by the patriarchal governments of modern Asian countries. These governments encourage women’s labor-force participation (whether domestically or abroad) to boost the national GNP of their countries but, at the same time, they continue to expect women to hold a subservient position in their households vis-à-vis their husbands and fathers (Afshar 1987; Brooks 2006; Chant and McIlwaine 1995; Lazar 2001; Roces and Edwards 2000, 2010; Sen 2004). Even as these pioneering women migrants break gender barriers when it comes to their labor migration, they do so by reinforcing the gendered and generational power hierarchy within their own homes.

Notes

1. I define “independent labor migration” as the setting forth of an individual from their hometown/village by themselves in order to seek work elsewhere, but without the benefit of a family member in the destination sponsoring them for a visa or a family member from their home accompanying them on their journey.
2. I use “family” and “household” interchangeably in this essay, though I recognize that they represent different living arrangements and family structures.
3. According to Hoang, “uncontested” situations are where the migrant decides to leave without any consultation with family members. In “conflictual” situations, migrants reported leaving despite unresolved disagreements with family members (2011b, 1446).
4. <http://www.census.gov.ph/statistics/survey/labor-force>.
5. <http://www.nscb.gov.ph/poverty/>.
6. <http://data.worldbank.org/country/philippines>.
7. The original sample consisted of 160 Filipino MDWs, including 21 US-based interviewees. However, the vast majority of the US-based interviewees had not left the Philippines intending to be domestic workers. Instead, most left the Philippines having

- been sponsored for permanent residence in the United States. As such, they would not qualify as independent women migrants, and so their responses are not included here.
8. Being married with children, belonging to an earlier cohort of migrants, and being less educated all positively correlated with the manifestation of overt familial resistance within my study sample. However, due to space constraints, I do not discuss this variation in familial resistance across my sample. I do so in a separate paper currently in preparation.
 9. There were some exceptions to this rule, for instance, in cases where husbands were deceased, unemployed, or earned significantly less than their wives.

About the Author

Anju Mary Paul is an assistant professor of sociology at Yale–NUS College in Singapore and an international migration scholar with a research focus on emergent patterns of international migration to, from, and within Asia. She studies both high- and low-skilled migration, from migrant domestic workers to nurses to bioscientists. She has published articles in the *American Journal of Sociology*, the *Journal of Ethnic and Migration Studies*, and *Ethnic and Racial Studies*.

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Eviction's Fallout: Housing, Hardship, and Health

Matthew Desmond, *Harvard University*

Rachel Tolbert Kimbro, *Rice University*

Millions of families across the United States are evicted each year. Yet, we know next to nothing about the impact eviction has on their lives. Focusing on low-income urban mothers, a population at high risk of eviction, this study is among the first to examine rigorously the consequences of involuntary displacement from housing. Applying two methods of propensity score analyses to data from a national survey, we find that eviction has negative effects on mothers in multiple domains. Compared to matched mothers who were not evicted, mothers who were evicted in the previous year experienced more material hardship, were more likely to suffer from depression, reported worse health for themselves and their children, and reported more parenting stress. Some evidence suggests that at least two years after their eviction, mothers still experienced significantly higher rates of material hardship and depression than peers.

Poor renting families are facing the worst affordable housing crisis in several generations. Millions of low-income households are devoting the majority of their income to housing costs, and millions are estimated to be evicted each year.

Historically, housing was central to the poverty debate. Slum dwelling, overcrowded and filthy housing conditions, and the development and expansion of housing programs were predominant in the study of urban life throughout the nineteenth and mid-twentieth century (e.g., Riis 1890; Park 1952; Foley 1980). And for much of the twentieth century, housing occupied a focal place in domestic policy. Until the 1980s, the Department of Housing and Urban Development's budget was second only to the Department of Defense's (Schwartz 2010, 45). But for the past several decades, housing has been relegated to the sidelines. Lyndon B. Johnson's War on Poverty placed the family, especially the black family, in the middle of the debate (Rainwater and Yancey 1967). In the wake of deindustrialization, the shuttered factory and chronic joblessness—issues raised by Wilson's *The Truly Disadvantaged* (1987)—took main stage. The poverty debate turned toward public assistance in the mid-1990s as President Clinton sought to “end

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welfare as we know it” (Edin and Lein 1997). More recently, the debate has focused on mass incarceration, with books like Western’s *Punishment and Inequality in America* (2006) and Alexander’s *The New Jim Crow* (2010). No one can deny the importance of these topics, but something fundamental is missing from the picture.

The poverty debate has not fully appreciated how housing dynamics are deeply implicated in creating and deepening poverty in America. Despite an impressive literature on inner cities and racial segregation and a rich tradition of community studies, research on housing and poverty is far less developed than the literature on the relationship between inequality and the family, employment, welfare, and the criminal justice system (Pattillo 2013). Yet, housing remains absolutely central to the lives of the poor. This is especially clear today, when the majority of poor renting families in America now devote over half of their income to housing costs (Desmond 2015). Extreme rent burden among low-income households necessarily makes them poorer. As households are forced to devote a larger portion of their income to housing expenses, their budget shares for food, school supplies, medication, transportation, and other necessities shrink (McConnell 2012; Newman and Holupka 2014). Owing to a shortage of affordable housing in urban areas, low-income families often move into substandard units, and housing problems have been linked to a wide array of negative health outcomes (Shaw 2004).

The affordable housing crisis also is a major source of residential instability among low-income families. In the absence of residential stability, it is increasingly difficult for low-income families to enjoy a kind of psychological stability, which allows people to place an emotional investment in their home, social relationships, and community (Oishi 2010); school stability, which increases the chances that children will excel in their studies and graduate (Temple and Reynolds 1999); or community stability, which increases the chances for neighbors to form strong bonds and to invest in their neighborhoods (Sampson 2012). As the severe housing burden among low-income households continues to rise, the number of households that experience acute residential instability owing to involuntary displacement from housing is likely to increase. If forced removal is becoming a common moment in the life course of poor Americans (Desmond 2012; Desmond, Gershenson, and Kiviat 2015), then investigating how eviction affects these families is critical to fully understanding the role housing dynamics play in driving health and economic disparities. Yet, researchers have neglected to identify the consequences of eviction.

This study corrects this oversight. Focusing on a population at heightened risk of eviction—low-income urban mothers—we examine the relationship between eviction and multiple outcomes by applying to a nationally representative and longitudinal data set several stringent statistical analyses. We find that eviction has negative effects on mothers in multiple domains. Compared to those not evicted, mothers who were evicted in the previous year experienced more material hardship, were more likely to suffer from depression, reported worse health for themselves and their children, and reported more parenting stress. Some evidence suggests that at least two years after their eviction, mothers still experienced significantly higher rates of material hardship and depression than peers. Our findings indicate that to fully understand the lives of disadvantaged women,

we should examine not only events related to work, welfare, and family, but also those related to housing, eviction being among the most consequential of them.

The Rise of Extreme Housing Burden among Poor Families

Today's affordable housing crisis is primarily the result of three factors: housing costs have soared, incomes of the poor have fallen or flatlined, and federal assistance has failed to bridge the gap.

Median monthly rent for vacant units in the United States was \$371 in 1990, \$483 in 2000, and \$633 in 2006 (all in current dollars)—an overall increase of 70 percent in 16 years (Downs 2008, 6; see also Collinson 2011). From 2001 to 2010, median rents increased by roughly 21 percent in Midwestern and Western regions, by 26 percent in the South, and by fully 37.2 percent in the Northeast. These advances far outpaced modest gains in median incomes, which in the 2000s rose by 6 percent for households headed by people with a ninth-grade education or less, 7.3 percent for those headed by high school graduates, and 12 percent by those headed by college graduates (Desmond 2015; see also Shierholz and Gould 2012).

During the years in which more and more renting families were in need of housing assistance, fewer and fewer new households were receiving it. Owing to cutbacks in budget authority, in recent years a growing portion of federal assistance has been dedicated to renewing existing subsidies, rather than to extending aid to new households. In an average year between 1981 and 1986, 161,000 additional households received subsidies; in an average year between 1995 and 2007, fewer than 3,000 did. As in years past, the vast majority of poor renters today do not benefit from federal housing programs (Schwartz 2010).

As a result of these structural changes, the number of families severely rent burdened has spiked in recent years. At least since the National Housing Act of 1937, which established America's public housing system, the public and its policymakers have believed that families should spend no more than 30 percent of their income on housing costs (Henderson 2013). Until recently, most renting households in the United States met this goal. But times have changed. Today, most renting households are not able to meet what long has been considered the standard metric of affordability, and spend more than 30 percent of their income on housing costs. At least one in five renter households in America now devotes at least half of its income to housing costs (Eggers and Moumen 2010).

Eviction in Poor Neighborhoods

The affordable housing crisis has placed millions of families at risk of eviction. New York City's housing courts process roughly 350,000 cases each year, the vast majority of which allege nonpayment of rent (Brescia 2009, 192). Research based on an analysis of Milwaukee court records found that one in 29 renter-occupied households in the city are evicted annually. With one in 14 renter-occupied households evicted through the court system annually, eviction is commonplace in Milwaukee's black neighborhoods (Desmond 2012). These estimates are limited to formal, court-ordered evictions. A recent study that captures multiple forms of

involuntary displacement—formal evictions (which are processed through the court) and informal evictions (which are not), landlord foreclosures, and building condemnations—found that between 2009 and 2011 one in eight Milwaukee renters experienced a forced move sometime in the previous two years (Desmond and Shollenberger 2013).

Low-income women—and mothers in particular—are at especially high risk of eviction. One of 11 mothers receiving welfare interviewed by Edin and Lein (1997, 53) reported having been evicted in the previous two years. “If our numbers were nationally representative,” the authors write, “1.3 million American children whose mothers relied on welfare were evicted over a two-year period... during the early 1990s.” Phinney et al. (2007) show that 20 percent of urban mothers in Michigan who were receiving cash welfare in February 1997 were evicted at some point between then and 2003. Desmond (2012) finds that in Milwaukee’s predominantly black inner-city neighborhoods, women are more than twice as likely to be evicted as men and, drawing on a survey of tenants appearing in housing court, also shows that among evicted tenants black women outnumber black men by 1.75:1, even after accounting for tenants excluded from the lease. One reason behind this discrepancy has to do with the fact that children can cause problems for landlords (e.g., noise complaints, lead poisoning). Indeed, among tenants who appear in eviction court, the likelihood of receiving an eviction judgment is highest for mothers with children, even after accounting for arrears (Desmond et al. 2013).

Eviction’s Fallout

Despite eviction’s prevalence in the lives of the urban poor, we know next to nothing about its impact on people’s lives. Social scientists and policymakers have all but ignored eviction—its antecedents, consequences, and social ramifications—rendering it the “hidden housing problem” (Hartman and Robinson 2003). The prevalence of eviction in the lives of low-income mothers, one of America’s poorest demographic groups, makes the lack of attention paid to it by researchers all the more troubling. Does eviction affect mothers’ material hardship and poverty? Their health? And which of its effects linger long after the event?

Before reviewing our hypotheses, let us provide a bit more detail about the eviction process. Evictions are landlord-initiated forced moves from rental property. (Foreclosures, on the other hand, are lending institution-initiated forced moves from owner-occupied property. Evictions tend to affect the urban poor; foreclosures, the working and middle class). Most evictions are attributed to non-payment of rent. A recent survey of tenants in eviction court found that one-third devoted at least 80 percent of their household income to rent, and that 92 percent received an eviction notice for falling behind (Desmond et al. 2013). It does not take a major life event (a death, a diagnosis) to cause severely housing burdened families to miss a rent payment; pedestrian expenses or setbacks—for example a reduction in work hours, or public benefits sanction—can cause families to come up short with the rent. When tenants miss a full payment, landlords show considerable discretion over whether to move forward with an eviction (Lempert and Ikeda 1970), and extra-financial considerations (the presence of children in the

household, for example) can influence their decision. Given the scope of the affordable housing crisis, many more families are in arrears than actually are evicted (Desmond 2012). These considerations, along with the frequency of eviction in low-income neighborhoods, reveal that many evictions are not necessarily the outcome of a drawn-out downward spiral or the result of a “more fundamental” cause having to do with tenants’ behavior or bad luck.

And irrespective of its underlying cause, there are many reasons to believe that eviction itself may be a considerably consequential event. For one, events leading up to the moment of forced removal—conflict with one’s landlord, multiple court appearances, looming uncertainty of the outcome—can consume tenants’ time and focus and can cause a good deal of stress (Manzo, Kleit, and Couch 2008). The actual moment of forced removal, moreover, also can be taxing. Families who receive an eviction judgment often are ordered to vacate in a matter of days; if the family is removed by sheriff deputies, its possessions are piled on the curb or confiscated by movers; many tenants, lacking legal counsel and confused by the eviction process, are caught off-guard when the eviction squad raps on their door and orders them to leave; and evicted families must find somewhere else to live very quickly and under considerable duress (Desmond 2012; Hartman and Robinson 2003). A further consideration is that tenants evicted through the court system carry that judgment on their record. Just as the mark of a criminal record can greatly affect one’s experiences on the job market (Pager 2007), the blemish of eviction can significantly influence one’s experiences on the housing market (Greiner, Pattanayak, and Hennessy 2013).

Poverty Effects

We hypothesize the consequences of eviction to be many and multidimensional. First, prolonged periods of homelessness may follow eviction (Burt 2001; Kleysteuber 2006).¹ During these periods, families’ belongings often are left behind or locked in storage by moving companies. The energy and resources that evicted tenants dedicate to securing subsequent housing and restoring a household often require them to forego other basic necessities, like warm clothing, food, or medical care. Additionally, a court-ordered eviction renders some voucher holders ineligible for federal housing assistance. And the mark of eviction on one’s record not only can prevent one from securing affordable housing in a decent neighborhood, it also can tarnish one’s credit rating (Greiner, Pattanayak, and Hennessy 2013). For these reasons, we hypothesize that *eviction will increase mothers’ material hardship*.

Additionally, eviction can prolong families’ residential instability, which begets economic instability (Desmond, Gershenson, and Kiviat 2015). A mother who does not know where she and her children will sleep the next night likely will be unable to maintain steady employment. If she is unemployed, securing housing after being evicted may take precedence over securing a job. If she is employed, the turmoil set off by eviction may affect her work performance and absenteeism, causing her to lose her job. Recent research has found the likelihood of being laid off to be 11 to 15 percentage points higher for workers who experienced an eviction or other involuntary move, compared to matched workers who did not

(Desmond and Gershenson 2015). These considerations lead us to hypothesize that *evicted mothers will experience higher levels of poverty*.

These proposed mechanisms suggest that the direct effect of eviction on material hardship will be longer lasting than the effect on poverty. Once a mother is able to regain a degree of residential stability post-eviction, she may refocus her energies on finding employment, transferring to a better job, or boosting her income by some other means. But the proposed factors through which eviction may lead to increased levels of material hardship—homelessness, the loss of possessions, and a legal eviction record—leave a deeper mark. Research has shown that homelessness has some long-term consequences (Sosin, Piliavin, and Westerfelt 2010); many low-income mothers will be unable to quickly replace their possessions if they were lost during the eviction; and the mark of an eviction will remain on a mother's record years after the event, with landlords classifying as "recent" evictions that happened in the past two to five years (Desmond 2012). Accordingly, we hypothesize that *the effect of eviction on mothers' material hardship will be resilient, lasting years after the event, while the effect on mother's poverty will be more short lived*.

Health Effects

The trauma of eviction and its aftermath also may have significant effects on mothers' health. Although very little is known about the effects of eviction on health outcomes, research documenting an association between foreclosure, housing instability, and health is beginning to appear (e.g., Burgard, Seefeldt, and Johnson 2012; Currie and Tekin 2011). Extended periods of homelessness that follow eviction can take a toll on one's physical health. Although evictions are concentrated in disadvantaged neighborhoods, families who are involuntarily displaced often relocate to neighborhoods with even higher levels of poverty and violent crime (Desmond and Shollenberger 2013). Severely distressed neighborhoods can negatively influence adults' and children's wellbeing (Sampson, Morenoff, and Gannon-Rowley 2002). What is more, evicted families desperate to secure housing often accept substandard living conditions (Desmond, Gershenson, and Kiviat 2015), which in turn can bring about significant health problems (Shaw 2004). Accordingly, we hypothesize that *evicted mothers will rate their health and the health of their children more poorly than their peers who avoided eviction*.

Mothers' mental health, too, might not be spared by eviction. Qualitative studies have shown that residents involuntarily forced from their homes experience psychological distress (Fried 1963; Manzo, Kleit, and Couch 2008). Recent studies have found that women who experienced a recent foreclosure were at significantly greater risk of depression (Osypuk et al. 2012). Moreover, studies have shown that trying events associated with poverty, such as forced displacement, can diminish a mother's capacity for affirming and supportive parenting and increase her tendency to act punitively and erratically toward her children (Bradley and Corwyn 2002). These considerations lead us to hypothesize that *mothers who have been evicted will be more likely to suffer from depression and will experience higher levels of parental stress*.

The effects of many of the social determinants on health discussed above appear to be most durable with respect to mental health outcomes. Shinn et al. (2008) found homelessness to have long-term associations with mental health but not with mother- or child-reported health. Experiencing involuntary housing loss might also result in “economic scarring” akin to what workers sometimes experience after involuntary job loss, scarring that has been linked to persistent depressive symptoms (Gallo et al. 2006). A large body of evidence in psychology has found that acute stressful life events can cause recurrent episodes of major depression (Kessler 1997). Eviction may be one such episode. For these reasons, we hypothesize that *the effect of eviction on mental health outcomes—and mothers’ depression in particular—will be resilient, lasting years after the event.*

Data and Methods

Data and Key Measures

We test our hypotheses by analyzing longitudinal data from the Fragile Families and Child Wellbeing Study (FFCWS), a survey that follows a birth cohort of new parents and their children. Initial interviews (Wave I) were conducted between 1998 and 2000 and contain information on 3,712 births to unmarried parents and 1,188 births to married parents from 20 US cities. Follow-up interviews were conducted at year one (Wave II), year three (Wave III), and year five (Wave IV). The survey oversampled unmarried mothers and contains a large sample of minority and disadvantaged women. The data include substantial information on the resources and relationships of parents and their effects on children.

We examine 2,676 mothers and children who were renting at the baseline wave and who persisted in the study through the fourth wave (when the child was approximately 5). Mothers who attrit before the fourth wave are less likely to be black and more likely to be Hispanic but otherwise are similar to mothers who persist on other characteristics and, importantly, are not more likely to have experienced an eviction by the third wave. To address missing data across all waves, we use Stata’s ICE command to execute multiple imputation (Royston 2009). The fraction of missing data varied across measures but rarely exceeded 8 percent. We include both treatment and outcome measures in the imputation equation but in our analyses do not use imputed outcomes (von Hippel 2007). We estimate 20 complete data sets for analysis.

At each wave, the FFCWS study asked mothers, “In the past 12 months, were you evicted from your home or apartment for not paying the rent or mortgage?”² Because the FFCWS followed the conventions of material hardship surveys by simply asking respondents if they had been evicted during a certain time period (e.g., Mayer and Jencks 1989), it underestimated (likely drastically) the number of respondents who experienced eviction. As previous work has shown (Desmond 2012), tenants often have misguided perceptions of eviction; many who were evicted do not realize (or admit) as much. This is why studies based on court records produce larger estimates of the scope of eviction than those based on self-reports. New survey techniques designed to capture the mechanisms driving families’ residential relocations—techniques that aim to record formal and informal

evictions—have found involuntary displacement to be common among low-income renters (Desmond and Shollenberger 2013). Because the FFCWS's eviction question likely did not capture all the evictions experienced by mothers in its sample, not only because some respondents who were involuntarily displaced likely reported otherwise but also because the data do not allow us to observe evictions that may have occurred when the child was between the ages of 1 and 2 and the ages of 3 and 4, other data are better suited to provide an estimate of the *frequency* of eviction among low-income families. However, because the FFCWS is a nationally representative, longitudinal data set that includes an item for eviction, it is an ideal data source to estimate the *effects* of an eviction. Our estimates of those effects are likely biased in a conservative direction, as some evicted families (who most likely experienced some of eviction's ramifications) were categorized as nonevicted.

Our event of interest is whether a mother experienced an "early eviction" (when the child was 0–1 or 2–3) or a "recent eviction" (when the child was 4–5). We examine the effects of recent and early evictions on six outcomes, each assessed during the fourth wave of the study (when the focal child was 5). *Material hardship* is a scale ($\alpha = .71$) composed of 10 dichotomous items that are summed and the resulting scale standardized such that higher values represent more hardship. The items measure a mother's ability to obtain basic necessities (e.g., food, clothing, medicine). *Income-to-poverty ratio* is a continuous ratio of the household's total income to the federal poverty threshold for a household of that size.³ *Mothers' and children's health status* was measured with the same question: "In general, would you say (your/your child's) health is...excellent, very good, good, fair, or poor?" Because the proportional odds assumption was not met, we dichotomize this outcome into "fair/poor" for both mothers and children. We rely on a dichotomous indicator to measure *depressive symptoms* in mothers. Mothers were asked a series of questions, focused on experiences in the previous 12 months, based on the Composite International Diagnostic Interview Short Form (CIDI-SF). Respondents were asked whether they had feelings of dysphoria (depression) or anhedonia (inability to enjoy what is usually pleasurable) in the past year that lasted for two weeks or more, and if so, whether the symptoms lasted most of the day and occurred every day of the two-week period. If so, they were asked more specific questions about: (a) losing interest, (b) feeling tired, (c) change in weight, (d) trouble sleeping, (e) trouble concentrating, (f) feeling worthless, and (g) thinking about death. Mothers were classified as probable cases of depression if they endorsed either dysphoria or anhedonia plus two of the other symptoms in the follow-up questions (leading to a CIDI-SF MD score of three or higher) (Kessler et al. 1998).⁴ Finally, *parenting stress* is an index composed of four questions asking mothers about parenting difficulties. To create the index, we summed responses to a scale, with higher values representing higher stress ($\alpha = .92$). Questions used to construct the material hardship and parental stress indices are reproduced in the appendix.⁵

Analytical Strategy

Seven percent of the sample experienced an eviction by the time the focal child was 5. Five percent experienced an "early eviction" (when the child was 0–1 or

2–3), and two percent experienced a “recent eviction” (when the child was 4–5). As we noted above, these numbers are very conservative estimates of the frequency of eviction. Some respondents ($N = 23$) experienced both early and recent evictions. To maximize sample size, all models estimating the effects of a recent eviction retained mothers who had experienced a prior eviction. Excluding repeat evictees from those models generated nearly identical results.

The effect of eviction on various outcomes is difficult to isolate, owing to a number of factors potentially related to both the likelihood of eviction and our outcomes. As we emphasized above, eviction is not always a predictable outcome of certain behaviors or chained events. Not all tenants who fall behind or break their rental agreement are evicted, and not all evictees fell behind or egregiously violated their rental agreement. Forced moves may be caused by landlord foreclosure, tenant-landlord disputes, building condemnations, and other factors exogenous to tenant behavior (Desmond and Gershenson 2015). Nevertheless, it is important to compare evicted and nonevicted families to determine whether there are multiple and meaningful differences between the two groups.

Significant differences between evicted and nonevicted respondents were detected along several key measures (see table 1). With respect to our outcome variables, mothers who experienced an eviction are more likely to be depressed and to experience higher parenting stress; they also report higher material hardship, lower income-to-poverty ratios, and worse health status for themselves and their child. Whether such differences are due to the eviction itself—or to characteristics that would predict both poorer outcomes and eviction—is the central question we test in our analyses.

Because respondents who have been evicted were found to be observationally different from those who have not been, standard regression techniques that estimate the average association of two variables across a large group of heterogeneous respondents would likely produce biased estimates of the effects of eviction, irrespective of the number of factors for which we controlled. More accurate and rigorous estimates of the effects of eviction can be generated by employing propensity score analyses. Propensity score estimation techniques apply an experimentalist logic to observational data, allowing us to compare mothers matched along a multitude of characteristics but who differ by whether they were exposed to a treatment (eviction). This study relies on two propensity score techniques: propensity score weighting and nearest-neighbor matching. Table 1 presents descriptive statistics for all variables included in our models, indicating which variables were used to predict propensity scores for both early and recent evictions. The goal of propensity score methods is to produce the best estimate of a treatment’s effects by comparing a treatment and control group that are as similar as possible, a similarity achieved when covariates across groups are “balanced” (Becker and Ichino 2002). Because for each type of eviction we retain the maximum number of covariates for matching that satisfied the balancing property, a significant number of demographic, neighborhood, and city variables were used to generate propensity scores (see table 1).

All respondents in our sample received a propensity score, the predicted probability of treatment. Once it was ensured that covariates in the treatment and control groups were balanced, the sample was restricted to the region of common support

Table 1. Descriptive Statistics, Fragile Families and Child Wellbeing Study, Renters at Baseline (*N* = 2,676)

	Full sample		Evicted		Not evicted		Recent evictions		Early evictions	
	% or mean		% or mean		% or mean		PS	ATT	PS	ATT
<i>Eviction measures</i>										
Ever experienced an eviction (<i>N</i> = 193)	0.07		–		–		–	–	–	–
Early eviction (child aged 0–3) (<i>N</i> = 147)	0.05		–		–		–	–	–	–
Midrange eviction (child aged 2–3) (<i>N</i> = 77)	0.03									
Recent eviction (child aged 4–5) (<i>N</i> = 64)	0.02		–		–		–	–	–	–
<i>Outcome measures (child age 5)</i>										
Material hardship (standardized)	0.00		0.82		–0.06***		–	–	–	–
Income-to-poverty ratio	1.59		1.07		1.62***		–	–	–	–
Mother's poor/fair health	0.16		0.27		0.15***		–	–	–	–
Child's poor/fair health	0.05		0.11		0.04***		–	–	–	–
Maternal depression	0.17		0.34		0.16***		–	–	–	–
Parenting stress	8.83		9.59		8.78**		–	–	–	–
<i>Shocks (in previous 12 months)</i>										
Father incarcerated (since child age 3)	0.22		0.40		0.21***		–	–	–	–
Mother's relationship dissolved	0.24		0.35		0.23**		–	–	–	–
Mother had an additional child	0.24		0.23		0.24		–	–	–	–
Sanctioned from welfare/TANF	0.03		0.08		0.03**		–	–	–	–
<i>Demographics</i>										
Race (Ref: White)	0.15		0.17		0.16					
Black	0.53		0.56		0.53					
Hispanic/other	0.32		0.27		0.32		x	x	x	x

Mother is foreign born	0.17	0.05	0.18***		
Mother's age at first birth	20.8	19.9	20.9*	x	x
Mother's parity – Wave I	1.19	1.35	1.18	x	x
<i>Socioeconomic status</i>					
Household income (\$10,000 s) – Wave I	2.37	1.95	2.40**		x
Household income (\$10,000 s) – Wave III	2.76	1.81	2.83***	x	x
Mother's education (Ref: Less than HS)	0.39	0.49	0.38		
HS	0.32	0.30	0.32	x	x
Some college +	0.29	0.21	0.30*		
Mother employed – Wave I	0.52	0.53	0.52		x
Mother employed – Wave II	0.50	0.49	0.50		
Mother employed – Wave III	0.53	0.44	0.54*	x	x
Father employed – Wave I	0.77	0.71	0.77		x
Father employed – Wave II	0.67	0.54	0.68***		
Father employed – Wave III	0.67	0.57	0.68**	x	x
Family does not have a credit card – Wave II	0.66	0.80	0.65***		
Family does not have a credit card – Wave III	0.68	0.83	0.67***	x	x
Child care cost per week – Wave II	44.1	40.9	44.3		
Child care cost per week – Wave III	68.6	32.8	71.4	x	x
Rent paid – Wave II	416.5	453.1	413.9		
Rent paid – Wave III	450.1	392.4	454.1*	x	x
Family owns a car – Wave II	0.41	0.37	0.41	x	x

(Continued)

Table 1. continued

	Full sample		Evicted		Not evicted		Recent evictions		Early evictions	
	% or mean	% or mean	% or mean	% or mean	% or mean	% or mean	PS	ATT	PS	ATT
<i>Family characteristics</i>										
Grandmother in household – Wave I	0.19	0.15	0.20				x	x	x	x
Number of adults in household – Wave I	2.2	2.1	2.2				x	x	x	x
Mother's relationship status										
(Ref: Married) – Wave I	0.18	0.08	0.19						x	x
Cohabiting – Wave I	0.42	0.54	0.41**						x	x
Single – Wave I	0.40	0.39	0.40						x	x
(Ref: Married) – Wave III	0.27	0.16	0.28				x	x		
Cohabiting – Wave III	0.33	0.34	0.33				x	x		
Single – Wave III	0.40	0.50	0.39**				x	x		
Father ever incarcerated – Wave II	0.36	0.51	0.35***							
Father ever incarcerated – Wave III	0.46	0.65	0.45***				x	x		
Legal paternity established – Wave II	0.74	0.68	0.75*				x	x		
Days per month father sees child – Wave II	22.5	19.7	22.7**							
Days per month father sees child – Wave III	21.5	18.9	21.7**				x	x		
Father is sometimes late with child support – Wave II	0.08	0.14	0.07**							
Father is sometimes late with child support – Wave III	0.15	0.23	0.14**				x	x		
Father has child support order for another child – Wave II	0.20	0.29	0.19**							
Father has child support order for another child – Wave III	0.22	0.26	0.21				x	x		

Mother has health problem that limits work – Wave II	0.08	0.13	0.08*		
Mother has health problem that limits work – Wave III	0.10	0.15	0.09**	x	x
Father has health problem that limits work – Wave II	0.08	0.10	0.08		
Father has health problem that limits work – Wave III	0.09	0.13	0.08*	x	x
Father has a drug or alcohol problem – Wave II	0.09	0.18	0.08***		
Father has a drug or alcohol problem – Wave III	0.10	0.18	0.09**	x	x
<i>Public assistance</i>					
Birth paid for with Medicaid – Wave I	0.70	0.78	0.70*	x	x
Receives SSI – Wave I	0.09	0.10	0.09	x	x
Receives SSI – Wave II	0.04	0.06	0.04		
Receives SSI – Wave III	0.06	0.05	0.06	x	x
Receives housing voucher/assistance – Wave I	0.19	0.21	0.19	x	x
Receives housing voucher/assistance – Wave II	0.20	0.18	0.20		
Receives housing voucher/assistance – Wave III	0.24	0.24	0.24	x	x
Receives public assistance of any kind – Wave I	0.43	0.54	0.42**	x	x
Lives in public housing – Wave I	0.16	0.15	0.16		x
Lives in public housing – Wave II	0.21	0.20	0.21	x	x
Lives in public housing – Wave III	0.17	0.14	0.17		
Received EITC – Wave II	0.35	0.31	0.35		
Received EITC – Wave III	0.45	0.52	0.44†	x	x
Receives assistance from any agency – Wave II	0.85	0.89	0.84		
Receives assistance from any agency – Wave III	0.79	0.88	0.78**	x	x
Sanctioned from welfare/TANF – Wave II	0.05	0.11	0.05**		

(Continued)

Table 1. continued

	Full sample		Evicted		Not evicted		Recent evictions		Early evictions	
	% or mean	% or mean	% or mean	% or mean	PS	ATT	PS	ATT		
Social support										
High instrumental support – Wave II	0.50	0.36	0.52***							
High instrumental support – Wave III	0.48	0.29	0.49***		x	x				
Frequency of religious attendance – Wave I	3.03	3.15	3.03				x		x	x
Frequency of religious attendance – Wave II	3.58	3.72	3.57							
Frequency of religious attendance – Wave III	4.26	4.41	4.24		x	x				
Neighborhood and city characteristics										
Number of years lived in neighborhood – Wave I	2.80	2.53	2.82		x	x	x		x	x
Neighborhood is unsafe at night – Wave I	0.20	0.27	0.19*		x	x	x		x	x
Number of moves between birth and age five	2.32	2.19	4.01***							
Owner-occupied housing, city	0.47	0.48	0.47		x	x	x		x	x
Average household size for renters, city	2.43	2.42	2.43		x	x	x		x	x
Rental housing vacancy rate, city	0.06	0.06	0.06		x	x	x		x	x
Median rent, city	621.2	608.0	622.2		x	x	x		x	x
Median number of rooms per unit, city	4.84	4.88	4.84		x	x	x		x	x
Median gross rent as % of household income, city	0.27	0.26	0.27†		x	x	x		x	x
N	2,676	193	2,483							

Note: Chi-squared or *t*-tests were used to compare evicted and nonevicted families. If a variable was used to calculate propensity scores for the propensity weighted models (PS) or the ATT matching models (ATT), it is indicated with an “x.” The shocks and residential mobility variables were not included in the weighting or matching equations, because only factors that are temporally prior to the treatment can be included in the propensity score model. Rather, they are included as adjustments after weighting and matching.

† $p < .1$ * $p < .05$ *** $p < .01$ *** $p < .001$

(which excluded two cases), meaning that the distribution of propensity scores for treatment and control cases overlapped. Within each imputed data set, each treated respondent was then matched with a control case, using nearest-neighbor matching with replacement. Next, we estimated the average treatment effect on the treated (ATT), which allows us to estimate the effect of an eviction on our outcomes by comparing the averages across treatment and control groups. Additionally, because matching is imperfect and differences between treatment and control cases may remain, we also present estimates of the ATT after further adjustment for covariates (Shafer and King 2008). Adjusting covariates involved estimating the ATT after matching and while controlling covariates (Rosenbaum 2002); this helps eliminate any residual bias between the two groups, post-matching.

Because we have a small number of treated cases (evictions) in our sample, standard matching techniques exclude a large number of respondents. Accordingly, we also develop a weighted propensity score model. This method increases our efficiency and statistical power by allowing us to retain the full sample and allows us to assess the robustness of our findings from propensity score matching. Here, we use propensity scores to calculate a weight for each respondent, thereby assigning all treated (evicted) cases a value of 1 and weighting all untreated cases according to their estimated propensities for eviction (Hirano and Imbens 2001). Formally, the weight is calculated as follows:

$$\omega(t, z) = t + (1 - t) * \check{e}(z) / (1 - \check{e}(z)),$$

where ω is the weight, t is a dichotomous treatment measure, and $\check{e}(z)$ represents the propensity score for each respondent. We then estimate linear or logistic regression models (depending on the outcome) treating propensity score weights as sampling weights. Respondents who were not evicted, but who have the highest propensities for eviction, are weighted more heavily.

Utilizing propensity score matching and weighting techniques allows us not only to present rigorous estimates of the effects of eviction but also to replicate our estimates in multiple models, reinforcing confidence in our findings. ATT models estimate the effect of an eviction by comparing the averages of the treatment and control cases. For linear outcomes, this involves direct comparisons with regression coefficients (as the latter also are averaged over respondents); for dichotomous outcomes, this involves calculating and comparing predicted probabilities for evicted and nonevicted respondents (which are more directly comparable to the unadjusted ATT estimates). To estimate the matching propensity scores, we utilize Stata's PSMATCH2 (Leuven and Sianesi 2003) command (nearest neighbor matching), revising the program to incorporate both correct standard errors for multiply imputed data sets as well as the ability to compute the ATT for dichotomous outcomes. Identical sets of covariates were used for the propensity score matching and weighting models. A number of additional covariates also were tested (not shown), and we retained the maximum number of covariates for both "early" and "recent" evictions that satisfied the balancing property.

The sets of covariates differ between models evaluating the effects of "early" and "recent" evictions because we can include only covariates for matching that

are temporally prior to the treatment (eviction). For example, we use household income at Wave I when calculating propensity scores for early evictions and household income at Wave III when calculating propensity scores for recent evictions. Also, we could not include residential mobility and life shocks when calculating propensity scores, as these variables are contemporaneous with our outcomes and occurred after the observed evictions. Instead, post-weighting and post-matching, we control for residential mobility—the number of moves a family has experienced between birth and age 5—and a set of contemporaneous (between child age 4–5) shocks: whether the father was incarcerated, whether the mother’s relationship had dissolved, whether the mother had an additional child, and whether the mother had been sanctioned from TANF.

Propensity score analyses allow us to address treatment selection conditional on observed covariates. To address possible bias introduced by unobserved factors, we employ two additional techniques to further assess the robustness of our findings. First, we use placebo regression, predicting our outcomes at Wave III instead of Wave IV for recent evictions; that is, the outcome precedes the treatment. This allows us to test whether the observed relationships from our propensity score models may be spurious. (Because our models for early evictions measure the effects of an eviction that occurred during the first wave of data collection, we were unable to test for bias with placebo regression. When the treatment is measured at Wave I, there is no scenario in which the outcome precedes treatment.) Second, to assess whether respondents’ stable but unobserved characteristics are influencing our observed relationships, we rely on OLS or logit models with fixed effects. These models investigate whether a recent or early eviction is associated with a change in our outcome measures between Waves III and IV. We account, additionally, for several time-varying factors across Waves III and IV to address the possibility of confounding due to time-varying observed characteristics.

Results

Tables 2 and 3 display the estimated effects of recent and early evictions, respectively. In both tables, model 1 presents a propensity score-weighted regression model without the contemporaneous shocks, and model 2 adds the shocks. Model 3 presents estimates from the ATT matching model without shocks, model 4 adds the shocks, and model 5 presents the same ATT estimates as in model 4 but further conditioned on a set of relevant covariates.⁶

Effects of a Recent Eviction

We turn first to results estimating the effect of a recent eviction on the wellbeing of mothers and children when the focal child is 5 (see table 2). Across all models, there is a large and robust relationship between a recent eviction and material hardship. Regardless of the estimation technique, respondents who experienced an eviction in the past year report around one standard deviation higher material hardship. We found eviction to be associated with reductions in the income-to-poverty ratio, although this relationship becomes insignificant in ATT models 3–5. In order to more directly compare the results from the logit models for our

Table 2. Effects of ■ Recent Eviction (child age 4–5) on Maternal and Child Wellbeing Outcomes at Child Age 5

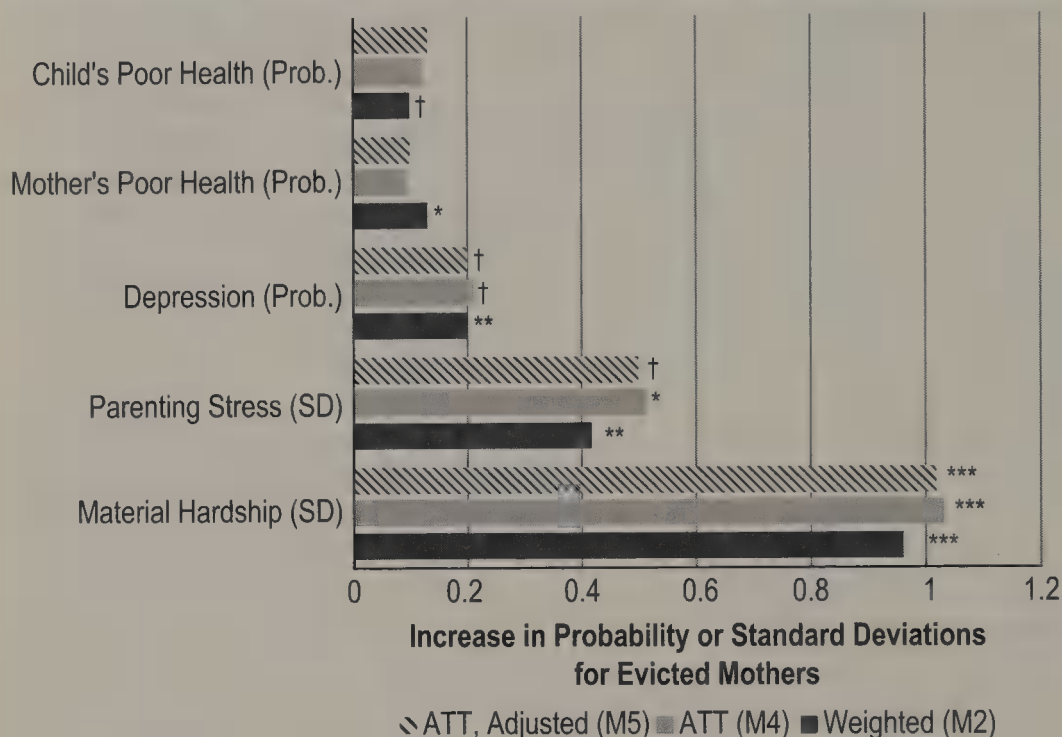
Outcome	Propensity score weighting (N = 2,676)		Propensity score matching (N = 122)		
	Model 1	Model 2	Model 3	Model 4	Model 5
	No shocks	With shocks	No shocks	With shocks	Regression adjusted, with shocks
	Coefficient		ATT		
Material hardship	0.99*** (0.16)	0.96*** (0.16)	1.06*** (0.23)	1.03*** (0.24)	1.02** (0.29)
Poverty ratio	−0.35** (0.11)	−0.30** (0.11)	−0.38 (0.31)	−0.34 (0.31)	−0.35 (0.33)
Parenting stress	1.19** (0.39)	1.18** (0.38)	1.42* (0.64)	1.45* (0.68)	1.41† (0.73)
	Difference in predicted probabilities, evicted vs. not evicted		ATT		
Mother's poor health	0.14* (0.07)	0.13* (0.06)	0.11 (0.10)	0.09 (0.11)	0.10 (0.11)
Child's poor health	0.10* (0.05)	0.10† (0.06)	0.11 (0.07)	0.12 (0.08)	0.13 (0.09)
Maternal depression	0.21** (0.07)	0.20** (0.07)	0.22* (0.11)	0.21† (0.11)	0.20† (0.11)

Note: Standard errors are in parentheses. All models control for residential mobility. ATT estimates represent the average treatment effect on the treated. Weighted models for mother's health, child's health, and depression are dichotomous outcomes estimated with logistic regression models; the difference in predicted probabilities for evicted and not evicted respondents are calculated for these outcomes to better compare to ATT estimates.

† $p < .1$ * $p < .05$ ** $p < .01$ *** $p < .001$

dichotomous outcomes with the propensity score weighted models, we calculate predicted probabilities from models 1 and 2 and assess the *average probability change* for evicted and nonevicted respondents. The weighted logit coefficient estimate from model 1 is equivalent to a .14 difference ($p < .05$) in the probability of mother's poor health, and a .10 difference ($p < .05$) in the probability of child's poor health, for evicted mothers compared to nonevicted mothers. This means, for instance, that for two mothers who are very similar, but only one experiences an eviction, the mother who is evicted is more than twice as likely to report that her child is in poor health. Adding the shocks in model 2 does not substantively change the estimates. These probability difference estimates from models 1 and 2 are very similar to the ATT estimates in models 3–5, demonstrating that the two different estimation techniques result in similar findings. Although the estimates are substantively similar, for models 3–5, the difference between evicted and nonevicted mothers on both health measures is not significant, which is likely an artifact of the much smaller sample sizes for these models.

Figure 1. Increase in probability or standard deviations for evicted mothers, estimated effects of recent eviction (models 2, 4, and 5 of table 2); † $p < .1$ * $p < .05$ ** $p < .01$ * $p < .001$**



Evicted mothers also were far more likely to report depression, equating to a predicted probability difference of approximately .21 across model specifications; or from model 1, about twice the likelihood (.47 versus .26). This effect drops to marginal significance in models 4 and 5. Finally, models 1 through 4 report a significant and large effect of an eviction on parenting stress. Mothers who experienced a recent eviction score more than one point higher on the parenting stress scale across specifications.

Figure 1 summarizes the statistically significant findings of models 2, 4, and 5. For five wellbeing outcomes, the figure graphs the increase in standard deviations or the difference in probability (for dichotomous outcomes), comparing mothers who experienced a recent eviction to otherwise similar mothers who did not. The pronounced effect of a recent eviction on mother's material hardship should not overshadow the fact that the effects on the other outcomes are substantively large as well. Evicted mothers report roughly half a standard deviation more parenting stress and worse self-reported health, and their probability of suffering from depression is approximately .2 higher than their peers.

We do not know the exact timing of the evictions, only that they occurred in the 12 months prior to the interview. This is unproblematic for the outcomes child's health status and parenting stress, which are asked about at the time of the interview. However, material hardship, mother's health status, depression, and income-to-poverty are asked about "in the prior 12 months." Accordingly, it is possible that a decline in the outcome would precede the eviction, rather than the other way around. To address this issue, we conducted a sensitivity analysis by

restricting evictions to those between child age 2–3, which we term “midrange evictions,” and estimated the exact same models presented in table 2. Doing so ensures that the eviction preceded the measurement of our outcome and is an especially stringent test given that the eviction took place as much as three years before the outcome was assessed. The results for midrange evictions are presented in the appendix (table A1) and show that experiencing a midrange eviction is associated with all of our outcomes except poor child health. Mothers who experienced a midrange eviction reported half a standard deviation higher material hardship between two and three years later, had lower income-to-poverty ratios, reported that their own health was poorer, were more likely to be depressed, and reported higher parenting stress than their nonevicted peers. In fact, the predicted probabilities for maternal health and depression show stronger differences than did our more recent eviction models. None of the ATT models for midrange evictions are significant, which we believe to be an artifact of the even smaller sample sizes used for the matching, as there were only 77 midrange evictions.⁷

Effects of an Early Eviction

We now turn to results estimating the effect of an early eviction (table 3). Across all models, an early eviction is associated with an increase in mothers’ material hardship. Each model reports an approximate effect size of one-third of a standard deviation higher on the material hardship scale. Models 1 and 2 ($p < .05$), and 3 and 5 ($p < .1$) also indicate that mothers who experienced eviction are more likely to report depression several years later. For example, model 2 estimates the probability of depression for mothers to be .31 for those who have experienced an early eviction and .20 for those who have not, a difference that is statistically significant ($p < .05$).

These analyses suggest, then, that eviction has long-term negative consequences for mothers’ material hardship and depression. However, it is important to recognize for both outcomes that these effects are reduced to insignificance or marginal significance in some of the matching models. With respect to the effect of an early eviction on material hardship, models 3 and 4 ATT estimates are marginally significant ($p < .1$). For maternal depression, models 3 and 5 are marginally significant and model 4 does not find significant effects of an early eviction on depression. Across all models, the magnitude of the effects of an early eviction on material hardship and depression are smaller than those of a recent eviction. This suggests (intuitively) that the influence of eviction on multiple outcomes shrinks over time and is felt less acutely—but is still felt—years after forced removal. Owing to the relatively small number of eviction cases in our sample, only large differences will be detected with significance in the matching models. We believe these factors help explain why the effects of an early eviction on material hardship and depression are of limited (or non-) significance in models 3–5.

Additional Sensitivity Analyses

Having accounted for dozens of observed covariates, we now ask: What about possible spuriousness introduced by unobserved factors? To first test for

Table 3. Effects of an Early Eviction (child age 0–1 or 2–3) on Maternal and Child Wellbeing Outcomes at Child Age 5

Outcome	Propensity score weighting (N = 2,676)		Propensity score matching (N = 236)		
	Model 1	Model 2	Model 3	Model 4	Model 5
	No shocks	With shocks	No shocks	With shocks	Regression adjusted, with shocks
	Coefficient		ATT		
Material hardship	0.36** (0.12)	0.31** (0.12)	0.30† (0.16)	0.28† (0.16)	0.33* (0.16)
Poverty ratio	−0.14 (0.11)	−0.09 (0.10)	−0.09 (0.16)	−0.05 (0.16)	−0.09 (0.15)
Parenting stress	0.15 (0.28)	0.05 (0.27)	0.49 (0.46)	0.45 (0.46)	0.62 (0.46)
	Difference in predicted probabilities, evicted vs. not evicted		ATT		
Mother's poor health	0.08† (0.05)	0.07 (0.05)	0.06 (0.06)	0.05 (0.06)	0.07 (0.06)
Child's poor health	0.01 (0.03)	0.01 (0.03)	0.01 (0.04)	0.01 (0.04)	—
Maternal depression	0.13* (0.05)	0.11* (0.05)	0.10† (0.06)	0.09 (0.06)	0.11† (0.06)

Note: Standard errors are in parentheses. All models control for residential mobility. ATT estimates represent the average treatment effect on the treated. Weighted models for mother's health, child's health, and depression are dichotomous outcomes estimated with logistic regression models; the difference in predicted probabilities for evicted and not evicted respondents are calculated for these outcomes to better compare to ATT estimates. The regression-adjusted ATT estimate for poor child health did not converge.

† $p < .1$ * $p < .05$ ** $p < .01$

spuriousness on account of omitted-variable bias in our models estimating the effect of a recent eviction, we performed a placebo regression sensitivity analysis. Rather than predicting outcomes at year five, this sensitivity analysis employs the same models to predict outcomes at year three. Because the outcome is prior to the treatment, there should be no relationship between the two. Results are presented in table 4. As in tables 2 and 3, the difference in predicted probabilities for evicted and nonevicted respondents are presented for the dichotomous outcomes. This test found no evidence of spuriousness between our treatment and outcomes, further reinforcing the robustness of the findings.

Finally, to assess whether any stable but unmeasured characteristics of families are influencing our estimated effects, we employ fixed-effects models, which hold constant respondents' traits that did not change over the course of the data collection. The results are presented in model 3 of table 5. In model 4, we further control for time-varying characteristics possibly associated with our outcomes,

Table 4. Placebo Regressions ($N = 2,676$)

Outcome	
Material hardship	0.42 (0.28)
Poverty ratio	-0.14 (0.23)
Parenting stress	0.33 (0.69)
Mother's poor health	0.12 (0.08)
Child's poor health	0.02 (0.09)
Maternal depression	0.06 (0.10)

Note: Standard errors are in parentheses. These models replicate model 4 of table 2 with year-three outcomes.

including household income, maternal and paternal employment, father's incarceration, mother's relationship dissolution, whether the mother had an additional child, monthly rent paid, whether the father is sometimes late with child support, and whether the mother has been sanctioned from welfare. If unobserved, stable characteristics were producing the effects of recent evictions, the fixed-effects model would report smaller or insignificant estimates. For material hardship, child's health, and parenting stress, we do observe smaller estimates—but the difference is slight and the substantive interpretation remains the same. In fact, all of the significant associations generated from the propensity score analyses are replicated in the fixed-effects models, and the size of the estimates is similar. These results indicate that our estimates of the effects of a recent eviction are attributed neither to stable but unobserved characteristics nor to a number of time-varying, observed covariates.

We also use a fixed-effects model to assess whether an early eviction was associated with a change between Waves III and IV in mothers' material hardship or depression, the two outcomes our propensity score analyses found to be significant. As we expected, given the results of our matching models, we found only a marginally significant relationship between an early eviction and material hardship changes between Waves III and IV. However, both fixed-effects models found a significant effect for an early eviction on changes in maternal depression, similar in magnitude to those from both propensity score analyses, further confirming our finding that eviction may leave a deep impression on mothers' mental health (see models 1 and 2 in table 5).

Finally, one might also ask if the same set of mothers experienced all the adverse outcomes or if some experienced one type of consequence while others experienced another. To address this point, we created an adverse factors scale, which represents the total number of adverse factors, derived from our six outcomes, experienced by mothers in the sample. For the continuous measures,

Table 5. Fixed-Effects Regression Models for an Early and a Recent Eviction’s Association with Changes in Outcomes between Waves III and IV (effective N = 2,676)

Outcome	Early eviction		Recent eviction	
	Model 1	Model 2	Model 3	Model 4
Material hardship	0.16 [†] (0.08)	0.15 [†] (0.08)	0.89*** (0.11)	0.87*** (0.11)
Poverty ratio	–	–	–0.36 (0.23)	–0.16 (0.24)
Parenting stress	–	–	1.07** (0.31)	0.99** (0.31)
Mother’s poor health	–	–	0.10** (0.04)	0.10* (0.04)
Child’s poor health	–	–	0.03* (0.02)	0.03 [†] (0.02)
Maternal depression	0.07** (0.02)	0.07** (0.02)	0.15*** (0.03)	0.14*** (0.03)

Note: Standard errors in parentheses. Models 2 and 4 include time-varying covariates (between Waves III and IV) for household income, maternal and paternal employment, father’s incarceration, mother’s relationship dissolution, whether the mother had an additional child, monthly rent paid, whether a father is sometimes late with child support, and whether the mother had been sanctioned from welfare. Because mother’s health, child’s health, and depression are dichotomous outcomes, we present the difference in predicted probabilities for evicted and not evicted respondents to better compare to our other estimates.

[†]*p* < .1 **p* < .05 ***p* < .01 ****p* < .001

we dichotomized each one to represent a “high” level relative to the rest of the sample. (For example, we characterized mothers reporting in the 75th percentile of material hardship as experiencing material hardship.) The adverse factors scale ranges from 0 to 6. Next, we assessed whether the pattern of adverse factors differed for evicted and nonevicted respondents; here, we pooled early and recent evictions for an “ever evicted” measure. We found that the modal number of adverse factors is 0 for nonevicted mothers and 2 for evicted mothers. About 13 percent of evicted mothers report experiencing three factors; 14 percent report experiencing four; 5 percent report experiencing five factors; and 2 percent report experiencing all six. Thus, it seems that while adverse experiences for evicted mothers most often occur in tandem, the patterning and degree of compounded adversity vary.

Discussion

This study yielded two important findings. We found, first, that eviction results in multiple and multidimensional negative consequences for mothers. Mothers who were evicted the previous year experienced higher levels of material hardship and parenting stress and were more likely to suffer from depression and to report their health and that of their children as being poor. The effects of a recent eviction on

multiple outcomes were substantively large, statistically significant across multiple specifications, and robust to hidden bias. The year following eviction is incredibly trying for low-income mothers. Eviction spares neither their material, physical, nor mental wellbeing, thereby undermining efforts of social programs designed to help them. The hardship of this difficult hour may in turn lead to additional hardships, such as relationship dissolution or selecting into a disadvantaged neighborhood (Desmond and Shollenberger 2013). Moreover, because the evictions we observed in our sample occurred at a crucial developmental phase in children's lives, we expect them to have a durable impact on children's wellbeing (Hertzman 2010).

Second, we found that the impact of eviction on some outcomes may be stubbornly resilient, enduring years after families were forced from their homes. We found some evidence that at least two years after their eviction mothers still experienced significantly higher rates of material hardship and depression than their peers. In our matching models, these effects were found to be marginally (or non-) significant. And our fixed-effects models reported a significant effect of an early eviction on maternal depression and a marginally significant effect on material hardship. These results imply that our findings regarding the long-term effects of eviction deserve our reserve. However, that the effects of an early eviction on material hardship and depression were found to be robust across multiple model specifications does suggest that eviction has long-term effects on these outcomes.

On some measures, eviction may not simply drop poor mothers and their children into a dark valley, a trying yet relatively short section along life's journey; it may fundamentally redirect their way, casting them onto a different, and much more difficult, path. If evicted mothers experience higher rates of depression several years after their forced removal, as our findings indicate, that suggests that eviction has lasting effects on mothers' happiness and quality of life. This in turn could affect their relationships with their romantic partners and children, kin and neighbors; could cause them to withdraw from social institutions, dampening their civic engagement and level of community embeddedness; and could sap their energy, preventing them from seeking or keeping gainful employment or participating fully in their children's development (Karp 1996). We also found some evidence that eviction has long-term effects on mothers' material hardship. Material hardship is a measure of the lived experience of scarcity. It assesses, say, if mothers experienced hunger or sickness because food or medical care was financially out of reach. Accordingly, our finding that evicted households have significantly higher rates of material hardship years after they were forced to move suggests that eviction may itself be a cause, not simply a condition, of poverty.

Our primary analyses incorporated a large number of variables potentially related both to eviction and to our outcomes. To isolate as much as possible the unique effects of early and recent evictions, we accounted for residential mobility, attributes of mothers' neighborhoods and cities, life shocks, health problems, socioeconomic status, social support, and many other family and individual characteristics. Doing so decreased the likelihood of spuriousness and increased our confidence that we identified the effects of eviction and not the effects, say, of residential instability, relationship dissolution, or some other event.

However, this study is not without limitations. Above, we explained the advantages of using the FFCWS to assess the effects of eviction, but one limitation of

this data set is that our findings, while tested across multiple methods for robustness, are based on a small number of eviction cases. Second, although the attrition rate in the FFCWS is fairly low, a number of mothers interviewed early in the study could not be located for subsequent interviews.⁸ The experiences of these mothers necessarily were excluded from our analyses. This is unfortunate since there is good reason to suspect that mothers who were not interviewed during later waves of the study were precisely those most likely to experience residential instability and homelessness, perhaps brought about by eviction. However, experiencing an early eviction was not a significant predictor of leaving the study by Wave IV.

To the extent that urban sociologists and city planners have focused on involuntary displacement from housing, they typically have done so by examining gentrification (Freeman and Braconi 2004; Newman and Wyly 2006). The act of forcing families from their homes, primarily through rent hikes, is central to the study of gentrification; and yet, curiously absent from this sweeping literature is rigorous empirical research that investigates whether displacement itself results in deep and lasting effects on adults and children. This study finds that eviction leads to economic hardship and health problems, but a thousand questions remain unanswered. Does displacement lead to family dissolution or job loss? By forcing families out of neighborhoods, does it sever network ties and the possibility of cultivating vibrant, civically active communities? The importance of documenting the fallout of involuntary displacement from housing has significant implications for current debates about gentrification. It is one thing if gentrification changes the character of urban neighborhoods but has little lasting effect on the displaced; it is quite another if forced displacement from housing has durable and significant effects on families' health and wellbeing.

But gentrification remains a narrow perspective through which to study involuntary displacement and residential instability among the urban poor. Most evictions take place in un-gentrifying neighborhoods (Desmond 2012) and are not the result of sudden rent hikes owing to neighborhood turnover but to missed rental payments, owing to the extreme degree to which many low-income households are rent burdened. Interest in gentrification far overshadows that on affordable housing; since 1980, for every social-scientific journal article in which "affordable housing" appears in the title, there are nearly three others featuring "gentrification." But investigating displacement among poor renters by studying gentrification is akin to documenting the causes of mortality by studying rare diseases, since in most cities gentrification is responsible for a very small fraction of involuntary moves (Kasarda et al. 1997). What is needed, then, is a sociology of displacement beyond gentrification, a new body of work that records the causes, dynamics, and consequences of forced removal from housing owing to the pedestrian workings of the low-income housing market in disadvantaged, segregated neighborhoods. By documenting the consequences of eviction, we have contributed toward such a project.

Although most low-income families live unassisted in the private market, most research on housing dynamics has to do with housing policy and programs. We know much more about public housing (which serves less than 2 percent of the population) than about inner-city landlords and their properties (which constitute

the bulk of housing for the ghetto poor) (e.g., Bratt, Stone, and Hartman 2006). We know much more about the effects of the “Moving to Opportunity” program, which served roughly 4,600 households, than the effects of eviction, likely experienced by millions of households each year. Evictions are but one aspect of the private rental market deserving of more research. The most direct connection between housing and poverty is the pervasiveness of severe rent burden in low-income communities. If poor families are spending the majority of their income to rent, what do they go without? Does the shortage of affordable housing affect social mobility opportunities or food scarcity, for example? Finally, sociologists could begin investigating how dynamics of the low-income housing market contribute to neighborhood dynamics. What role does landlord screening play in the concentration of disadvantage or criminality in some inner-city areas? What role do evictions play in high residential turnover and community destabilization? By pursuing questions like these, research focused on the low-income private rental market, that cut of the country in which the majority of poor families are found, would help pull housing back to the center of the poverty debate, where it belongs.

By providing evidence that eviction brings about a variety of negative outcomes, this study underscores the need for policymakers to focus their attention on forced removal. If eviction is linked to economic and health disparities, then effective eviction-prevention initiatives could go a long way toward addressing these enduring problems. Relatedly, because we find that evicted mothers and their children were more likely to suffer from health problems, directing eviction-prevention aid upstream potentially could lower healthcare costs incurred downstream.

Notes

1. But this is not universally the case. A survey of tenants in housing court who received eviction judgments found that 14 percent planned to live with kin or friends, 15 percent had found another apartment, 12 percent were planning on staying in a hotel or shelter or on the street, and the remaining 53 percent simply did not know where they would stay after their eviction (Desmond 2012). Sometimes eviction results in homelessness—itself coming in many different forms: doubling up, living on the street, taking refuge in a shelter—and sometimes it does not. Studying the effects of eviction is not the same thing as studying the effects of homelessness.
2. This wording does not allow us to distinguish between tenants who were evicted formally (and carry the mark of an eviction on their record) and those who were evicted informally (and are spared an eviction record).
3. Our income-to-poverty measure is based on the federal poverty threshold for the year prior to each survey wave.
4. Our results are robust to varying the cut-point for the depression scale as well as to negative binomial models estimating the number of depressive symptoms respondents reported.
5. In supplementary analyses, we constructed fixed-effects models that accounted for the Wave III outcomes. Additionally, we replicated our regression models by including Wave III outcomes as covariates. Doing so did not significantly alter our main results. Because our fixed effects models account for unobserved confounders and assess

changes in our outcomes between Waves III and IV, we have not displayed those results here. They are available upon request.

6. For “early evictions,” model 5 adjusts, post-matching, for race/ethnicity, mother’s nativity, whether the father had ever been incarcerated, parity, whether a grandmother lived in the household at the time of the birth, how many adults live in the household, how long the mother has lived in her neighborhood, whether she receives housing assistance, whether she feels safe in her neighborhood, whether she lived in public housing at Wave I, maternal and paternal employment status, relationship status at Wave I, whether the family received any public assistance at Wave I, whether the family received SSI or unemployment at Wave I, mother’s education, whether the family paid for the birth with Medicaid, the total household income at Wave I, whether paternity had been established, and the mother’s age at her first birth. For “recent evictions,” Model 5 adjusts, post-matching, for race/ethnicity, parity, the number of adults in the household, whether the father had ever been incarcerated, whether a grandmother lived in the household at the time of the birth, maternal and paternal employment status, mother’s education, relationship status at Wave III, how long the mother had lived in her neighborhood, whether she received housing assistance, whether she feels safe in her neighborhood, whether she lived in public housing, whether the family received any public assistance at Wave I, whether the family owned a car at Wave II, monthly rent paid, monthly childcare costs, whether the father is ever late with child support, whether the mother reports high social support, whether the family received the EITC, whether the mother or father had any health problems that affected their ability to work, total household income at Wave III, how many days per month the father sees the child, whether legal paternity had ever been established, the mother’s age at first birth, and whether the family has a credit card at Wave III.
7. We also conducted a sensitivity test by restricting evictions to those between child age 0–1, which we term “very early evictions.” The results (not shown) were similar to the results for “midrange evictions,” though the associations were generally smaller in magnitude, as would be expected.
8. Eighty-nine percent of the original sample of mothers were re-interviewed in Wave II, 86 percent in Wave III, and 85 percent in Wave IV.

Appendix

Material Hardship Scale Items

Mothers were asked if in the past twelve months they did “any of the following because there wasn’t enough money.”

1. Did you receive free food or meals?
2. Was (CHILD) ever hungry, but you just couldn’t afford more food?*
3. Were you ever hungry, but didn’t eat because you couldn’t afford enough food?*
4. Did you not pay the full amount of a gas, oil, or electricity bill?
5. Was your gas or electric service ever turned off, or the heating oil company did not deliver oil, because there wasn’t enough money to pay the bills?
6. Did you borrow money from friends or family to help pay bills?
7. Was there anyone in your household who needed to see a doctor or go to the hospital but couldn’t go because of the cost?

8. Have you cut back on buying clothes for yourself?
9. Have you worked overtime or taken a second job?
10. Was your telephone ever disconnected by the telephone company because there wasn't enough money to pay the bill?

* These items were asked in the Wave IV follow-up only.

Parenting Stress Items

Mothers were asked whether they strongly agreed, somewhat agreed, somewhat disagreed, or strongly disagreed with the following statements.

1. Being a parent is harder than I thought it would be.
2. I feel trapped by my responsibilities as a parent.
3. I find that taking care of my children is much more work than pleasure.
4. I often feel tired, worn out, or exhausted from raising a family.

Table A1. Effects of ■ "Midrange" Eviction (child age 2–3) on Maternal and Child Wellbeing Outcomes at Child Age ■

Outcome	Propensity score weighting (N = 2,676)		Propensity score matching (N = 154)		
	Model 1	Model 2	Model 3	Model 4	Model 5
	No shocks	With shocks	No shocks	With shocks	Regression adjusted, with shocks
	Coefficient		ATT		
Material hardship	0.56** (0.17)	0.53** (0.17)	0.33 (0.25)	0.31 (0.26)	0.35 (0.29)
Poverty ratio	−0.32* (0.15)	−0.28† (0.15)	−0.51 (0.40)	−0.43 (0.40)	−0.06 (0.23)
Parenting stress	0.95* (0.40)	0.89* (0.40)	0.66 (0.67)	0.65 (0.68)	0.56 (0.79)
	Difference in predicted probabilities, evicted vs. not evicted		ATT		
Mother's poor health	0.21** (0.07)	0.20** (0.07)	0.13 (0.09)	0.13 (0.10)	0.14 (0.11)
Child's poor health	0.05 (0.05)	0.04 (0.05)	—	—	—
Maternal depression	0.25** (0.08)	0.24** (0.08)	0.15 (0.09)	0.14 (0.09)	0.16 (0.10)

Note: All models control for residential mobility. ATT estimates represent the average treatment effect on the treated. Weighted models for mother's health, child's health, and depression are dichotomous outcomes estimated with logistic regression models; the difference in predicted probabilities for evicted and not evicted respondents are calculated for these outcomes to better compare to ATT estimates. ATT models for child's health would not converge.

† $p < .1$ * $p < .05$ ** $p < .01$ *** $p < .001$

About the Authors

Matthew Desmond is an Assistant Professor of Sociology and Social Studies at Harvard University. Recently, he has published on eviction, poverty, and housing in the *American Journal of Sociology*, *American Sociological Review*, *Social Service Review*, and *Social Forces*.

Rachel Tolbert Kimbro is an Associate Professor at Rice University and the founding director of the Urban Health Program at the Kinder Institute for Urban Research. Her research focuses on the impact of social contexts like neighborhoods and families on health and wellbeing. She has published articles in these areas in *Social Problems*, *Journal of Marriage and the Family*, *Journal of Health and Social Behavior*, *Health Affairs*, and *Social Science and Medicine*.

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Housing Policy and Urban Inequality: Did the Transformation of Assisted Housing Reduce Poverty Concentration?

Ann Owens, *University of Southern California*

Poverty concentration reflects long-standing inequalities between neighborhoods in the United States. As the poverty concentration paradigm gained traction among policymakers and social scientists, assisted housing policy was overhauled. New assisted housing programs introduced since 1970 have dramatically reduced the geographic concentration of assisted housing units, changing the residential location of many low-income residents. Was this intervention in the housing market enough to reduce poverty concentration? Using national longitudinal data, I find that the deconcentration of assisted housing from 1977 to 2008 only modestly reduced poverty concentration in the 100 largest metropolitan areas. The results are driven by the deconcentration of assisted housing after 2000, when policies had a greater focus on dispersal of assisted housing to low-poverty neighborhoods. My results suggest that even a substantial shift in housing policy cannot make great strides in deconcentrating poverty given the existing landscape of durable urban inequality. Assisted housing policy exists alongside many other structural forces that cluster poor residents in neighborhoods, and these factors may limit its ability to reduce poverty concentration. Moreover, new housing programs rely on the private market to determine the location of assisted units, and the enduring place hierarchy among neighborhoods may influence both where assisted housing is located and its effect on the residential choices of non-assisted residents in ways that undermine poverty deconcentration.

Poverty concentration has persisted in American cities for decades, and it is one aspect of inequality between neighborhoods that shapes the life chances of residents (Wilson 1987; Jargowsky 1997, 2003). The causes and consequences of poverty concentration—the degree to which poor residents are clustered in

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neighborhoods—have been widely discussed. Over the past 40 years, as attention to poverty concentration grew, there has been a major shift in how some poor residents are housed. Federal assisted housing policy geographically deconcentrated its housing units from large public housing developments in just a few neighborhoods to housing vouchers for use in any neighborhood and smaller housing developments in many more neighborhoods than traditional public housing.¹ Housing assistance provides relief for individual families, but this massive policy shift may also have impacts on neighborhoods and metropolitan areas because it changes the location of some low-income residents. Did this change in assisted housing policy reduce the concentration of poverty? Little research assesses this possibility.

Urban sociologists have considered how structural features of cities and metropolitan areas shape neighborhood inequality since the Chicago school (Park and Burgess 1925). Rather than examining neighborhoods' effects on individuals, this line of research focuses on how and why rates of economic and social characteristics—poverty rates, for example—vary across neighborhoods (Sampson 2008). Residential sorting between neighborhoods by economic and social characteristics happens in part because neighborhoods differ in their real estate values, but interventions in the market can prioritize special-use goals, like racial or economic integration (Logan and Molotch 1987). Assisted housing policy is one such intervention that may have equalizing effects on urban neighborhoods since it determines the residential locations of some low-income families. However, while assisted housing policy is a state intervention, its programs operate within the private market, against a landscape of long-standing inequality between neighborhoods that may prevent this policy shift from reducing poverty concentration. Recent research identifying some neighborhoods as “poverty traps” emphasizes that policy interventions may be necessary to reduce durable neighborhood inequality and poverty concentration (Sampson and Morenoff 2006; Sampson 2009, 2012), but has the geographic deconcentration of assisted housing been enough?

In this article I test this possibility, and I find that the geographic deconcentration of assisted housing, the result of several housing programs initiated since the 1970s, only modestly reduced metropolitan-area poverty concentration from 1980 to 2009. Even though a substantial policy shift occurred, its effectiveness in reducing poverty concentration was tempered by the existing context of durable urban inequality. The study of housing in sociology has faded or been folded into neighborhood studies over the past few decades (Pattillo 2013), and this article returns housing to the forefront of an analysis of urban inequality.

Poverty Concentration and Urban Inequality

A large body of social science research examines the consequences of living in high-poverty, low-resource neighborhoods, particularly for children. While still debated, many reviews of the literature conclude that there are negative impacts of concentrated poverty on educational, health, economic, and social outcomes (Brooks-Gunn, Duncan, and Aber 1997; Leventhal and Brooks-Gunn 2000; Sampson, Morenoff, and Gannon-Rowley 2002; Durlauf 2004; Galster 2012;

Sastry 2012; Sharkey and Faber 2014). Many neighborhoods remain high (or low) in poverty for decades, with little change in the hierarchy of neighborhoods within metropolitan areas (Sampson 2012). Neighborhood stratification remains fairly stable due to subsequent generations living in the same types of places (Sharkey 2008), racial discrimination preventing individuals' mobility (Massey and Denton 1993), political and economic forces like prolonged disinvestment in neighborhoods, and cultural forces like symbolic values attached to neighborhoods (Logan and Molotch 1987). State interventions like assisted housing policy, then, may be necessary to disperse poor residents to lower-poverty neighborhoods and break the cycle of neighborhood poverty. However, assisted housing may not be sufficient to reduce poverty concentration given the other stratifying processes that lead to poverty concentration.

Many structural factors contributed to rising poverty concentration through the 1980s: economic restructuring that reduced low-income workers' wages and led to economic growth outside central cities (Wilson 1987; Kasarda 1990); the exodus of the black middle class from urban neighborhoods (Wilson 1987; Quillian 1999); persistent racial segregation in housing markets and employment (Massey and Denton 1993; Quillian 1999); and exclusionary zoning in high-income communities (Goetz 2003; Jargowsky 2003; Rothwell and Massey 2010). Other factors account for the decline in poverty concentration in the 1990s: a booming economy, gentrification and redevelopment of central cities, improvements in poor urban residents' incomes due to welfare reform and the Earned Income Tax Credit, and a decline in crime rates, which attracted higher-income residents to the city (Jargowsky 2003; Ellen and O'Regan 2008).

Assisted housing has also been noted as an explanation for poverty concentration. New public housing projects led to increases in poverty rates in both neighborhoods where public housing was located and surrounding neighborhoods prior to 1990 (Hirsch 1983; Massey and Kanaiaupuni 1993; Holloway et al. 1998; Schill and Wachter 1995; Carter, Schill, and Wachter 1998). New housing programs that deconcentrated assisted housing were hypothesized as potential explanations for the decline in the number of high-poverty neighborhoods and the concentration of poor residents in them during the 1990s (Ellen and O'Regan 2008), but little evidence tests this claim. In the next section, I describe how assisted housing policy was shaped by and addresses the poverty concentration paradigm before discussing how the deconcentration of assisted housing might affect poverty concentration.

Changes in Assisted Housing Policy

Since the 1960s, the Department of Housing and Urban Development (HUD) has developed assisted housing programs that may reduce poverty concentration (and racial segregation) by facilitating the movement of low-income residents into many more neighborhoods than the high-poverty, majority-minority neighborhoods where public housing historically existed. This policy shift was spurred by concerns over growing problems in American inner cities documented by the Kerner Commission and other federal reports, as well as evidence that public housing itself created racially and economically segregated communities (Hirsch

1983; Goering 2005; Galster 2013). The “first generation” of dispersal programs in the early 1970s included the creation of scattered-site public housing units, sited in lower-poverty and more racially integrated neighborhoods, and the Section 8 program (1974), which created housing vouchers for use on the private rental market and subsidized project-based units in privately owned buildings (Goetz 2003). The need to address segregation was amplified by legal decisions holding housing authorities accountable for perpetuating racial segregation (e.g., the *Gautreaux* case in Chicago), and housing vouchers and scattered-site public housing were used in these cases to provide residents access to racially integrated neighborhoods. The Low Income Housing Tax Credit (LIHTC) program (1986) created new assisted housing options by providing tax credits for affordable housing developments. HUD also ceased building family public housing projects in the late 1960s.

By the late 1980s, public housing was occupied primarily by very poor and minority families, and some large developments were home to crime and other social problems. Policymakers increasingly saw assisted housing as intertwined with the problem of poverty concentration in American inner cities. The ascendancy of the poverty concentration framework, brought to policymakers’ attention by Wilson (1987) and other social science research, motivated a “second generation” of deconcentration programs focused on tenant dispersal (Goetz 2003). Over the past two decades, policymakers have used tenant-based vouchers and overhauled project-based programs to encourage mobility out of high-poverty neighborhoods. Policy changes, culminating in the Quality Housing and Work Responsibility Act (QHWRA) of 1998, promote economic integration through voucher use by allocating more vouchers to the poorest residents and allowing vouchers to be used in jurisdictions other than the issuing one (e.g., permitting moves to the suburbs). There are also more than 50 voucher mobility programs that incorporate mobility counseling or restrict mobility to low-poverty neighborhoods (Goetz 2003). One demonstration program was Moving to Opportunity (MTO), an experimental program that randomly selected participants from nearly 5,000 public housing families in five cities to receive vouchers redeemable in low-poverty neighborhoods (Briggs et al. 2010; Goering 2005). On the project-based side, HUD funded the demolition and redevelopment of public housing into mixed-income communities, in part through the HOPE VI program (1993). Over 250,000 public housing units (out of 1.4 million) have been demolished since the mid-1990s, more than half through HOPE VI (Schwartz 2010).

Since the 1970s, then, HUD programs have dispersed low-income assisted households. What were the intended outcomes of this policy shift? New housing programs were motivated in part by cost and efficiency goals of providing decent, safe, and affordable housing in an increasingly neoliberal policy context (Goering 2005; Khadduri and Wilkins 2008; Galster 2013). Public housing had become expensive to maintain and modernize, and limited capital funds were available, so policymakers increasingly relied on private-market landlords to produce affordable housing (Schwartz 2010). Arguably, HUD has not undertaken a systematic or comprehensive approach to poverty deconcentration or racial integration (Galster 2013; Goering 2005), instead creating a patchwork of programs to house low-income assisted residents.

However, new housing programs explicitly set out to create income diversity within project-based assisted housing, to disperse voucher users to lower-poverty neighborhoods, and to disperse project-based units into privately owned buildings, all of which affect both assisted tenants and the neighborhoods losing or receiving assisted units (Khadduri 2001; Goetz 2003). The policies directly affect the geographic location of low-income assisted households, and they have become deconcentrated. In 1977, fewer than 10 percent of Census tracts nationwide had any assisted housing. By 2008, more than 85 percent of tracts had at least one assisted unit, typically a voucher used in the neighborhood (Owens 2012). In addition to being located in many more neighborhoods, new assisted housing is now located in somewhat lower-poverty neighborhoods than public housing (Newman and Schnare 1997; Pendall 2000; Devine et al. 2003; Schwartz 2010; Galster 2013).

Beyond its effects on assisted residents' neighborhood attainment, the geographic dispersal of assisted housing has community- and metropolitan-area-level impacts, affecting neighborhoods that formerly had public housing, neighborhoods where new assisted housing is located, and the distribution of low-income families across neighborhoods within metropolitan areas (Goetz 2003, 2010, 2013). Even if HUD has not undertaken a comprehensive or systematic single policy aimed at economic and racial integration, assisted housing has been the largest mover of low-income households over the past several decades, with implications for poverty concentration that must be studied.

The Impact of Assisted Housing on Poverty Concentration

Assisted housing is a state intervention in the housing market to achieve a special-use goal of housing low-income residents and, as dispersal programs were developed, to facilitate mobility to lower-poverty neighborhoods (Logan and Molotch 1987). Assisted housing may affect poverty concentration because it affects where people live—the “structural flows” (Sampson and Sharkey 2008) of low-income assisted residents into and out of neighborhoods. However, mobility flows occur within a durable system of neighborhood inequality, with neighborhoods having different affordability and desirability for various groups. Therefore, if assisted housing affects neighborhood desirability or affordability, it may also shape the residential choices of high- and low-income residents *not* living in assisted housing. Assisted housing will reduce poverty concentration only if it can overcome existing social and economic forces that cluster poor and non-poor residents in different neighborhoods.

First, assisted housing has direct effects on where low-income assisted residents live. Assisted housing is not an entitlement program; the proportion of poor residents living in assisted housing nationwide was about 10 percent in 1977 and 24 percent in 2008 (author's calculations), and this varies by metropolitan area and neighborhood.² Although a minority of poor residents live in assisted housing, its potential to reduce poverty concentration has increased over time, since the number of assisted units nearly tripled since 1977, accounting for the location of more poor residents. However, not all assisted residents are poor—nearly 90 percent of public housing and voucher users but only about 50 percent of LIHTC

residents have incomes below the federal poverty line (O'Regan and Horn 2013; Schwartz 2010). Therefore, the first thing to consider is whether enough poor families live in assisted housing for its deconcentration to have an impact on poverty concentration.

I conducted a simple simulation of what poverty concentration would be if all poor assisted households lived in neighborhoods with poverty rates below 10 percent, described in the appendix. I find that, if nothing else changed, poverty concentration would be lower if all poor assisted households lived in low-poverty neighborhoods. The average poverty rate in a poor person's neighborhood would be about 4 percentage points lower, and the proportion of poor residents living in neighborhoods with poverty rates over 40 percent would be 5 to 6 percentage points lower. The touted decline in poverty concentration during the 1990s was also 5 percentage points (Jargowsky 2003), so this is a nontrivial impact. Assisted housing, then, directly affects enough poor residents to impact poverty concentration.

Assisted housing may reduce poverty concentration through both project-based and voucher programs. Public housing demolition may displace some poor residents to lower-poverty neighborhoods with replacement vouchers. However, public housing demolition does not always lead to an outflow of poor residents—some residents use replacement vouchers in the same neighborhood, and some exit assisted housing but stay in the neighborhood (Goering, Stebbins, and Siewert 1995). Other project-based programs and vouchers may also provide low-income residents opportunities for mobility to low-poverty areas. However, while assisted housing units are located in lower-poverty neighborhoods than public housing was 40 years ago, new programs do not guarantee the movement of poor residents to low-poverty neighborhoods (Pendall 2000; Devine et al. 2003; Kingsley, Johnson, and Pettit 2003; DeLuca, Garboden, and Rosenblatt 2013). In 2000, the majority of public housing and Section 8 project-based units and over a third of voucher and LIHTC units were still located in neighborhoods with poverty rates over 20 percent (Schwartz 2010). New housing programs rely on the private market to provide units, and landlords or developers in low-poverty neighborhoods may not participate if they can get higher market rents. Low-poverty neighborhoods may lack sufficient rental-housing supply or have zoning regulations preventing the types of buildings where assisted units are located (Katz and Turner 2008; DeLuca, Garboden, and Rosenblatt 2013).

The second way assisted housing may impact poverty concentration is through indirect effects on non-assisted residents. When public housing is demolished, any declines in poverty rates due to the outflow of poor assisted residents could be offset by non-assisted poor residents moving in or by non-poor residents leaving or avoiding these neighborhoods. Years of disinvestment and the stigma of public housing relegate these neighborhoods to the bottom of the place hierarchy, and demolition alone may not make them attractive to non-poor residents. Poverty rates declined in neighborhoods surrounding several HOPE VI sites (Holin et al. 2003), but poverty rates did not decline, on average, in all neighborhoods losing assisted housing, as many neighborhoods did not experience redevelopment (Kucheva 2013; Owens forthcoming).

The dispersal of assisted housing units to lower-poverty neighborhoods will decrease poverty concentration only if non-poor residents remain in or move into these neighborhoods. If many non-poor residents leave or avoid neighborhoods as assisted residents move in, this could create new high-poverty neighborhoods, rather than dispersing poverty. Non-poor residents may leave or avoid neighborhoods with assisted housing because it signals minority racial composition or a long history of disadvantage (Helms 2003). Poverty rates increased modestly in neighborhoods that gained assisted housing after 1977, due to both the in-migration of poor residents and the avoidance of non-poor residents (Freeman 2003; Kucheva 2013; Owens forthcoming).

Most research on the impact of assisted housing focuses on neighborhood poverty rates rather than metropolitan-area poverty concentration. Two studies use simulations to estimate how the changing location of assisted housing impacts poverty concentration in metropolitan areas and counties, and neither finds that the deconcentration of assisted housing reduced poverty concentration (Quillian 2005; Kucheva 2011). However, Quillian (2005) examines only public housing demolition, and Kucheva (2011) extrapolates from neighborhood-level models in only eight counties. Neither study measures assisted housing deconcentration at the metropolitan-area level directly for all housing programs.

Assisted housing programs are one structural force that may influence residential mobility of both poor and non-poor residents between persistently unequal neighborhoods. The impact of the massive changes in assisted housing policy on poverty concentration remains understudied, especially at the metropolitan-area level. Evidence from past research suggests that the dispersal of assisted housing has not reduced poverty rates in neighborhoods where it was formerly located, and that poverty rates may increase in neighborhoods where assisted units are now located. Combined with the limited evidence from two simulation studies, I hypothesize that the deconcentration of assisted housing has done little to deconcentrate poverty in the face of long-standing neighborhood stratification.

Data and Methods

I use national data on the location of assisted housing and neighborhood poverty to test whether the deconcentration of assisted housing has reduced poverty concentration at the metropolitan-area level from 1980 to 2005–09. Data on poverty as well as demographic control variables come from the 1980 and 2000 Decennial Censuses, normalized to 2000 tract boundaries through the Neighborhood Change Database (Geolytics 2003), and the 2005–09 American Community Survey.³ I define metropolitan areas using 1999 Metropolitan Statistical Area (MSA) definitions for consistency over time.⁴ I limit analyses to the 100 most populous MSAs as of 2009 because past research shows that poverty concentration is lower and follows different trends among smaller MSAs (Kneebone, Nadeau, and Berube 2011). Results for all 331 MSAs are included in the appendix.

Data on the location of assisted housing come from HUD's *A Picture of Subsidized Households* (PSH) (1977, 2000, 2008). The 1977 file provides the location only of public housing projects. I geocoded project addresses from HUD's "Subsidized Housing Projects' Geographic Codes, Form HUD-951" to assign

projects to tracts. Due to missing data, I could not geocode ~10 percent of public housing units among the 100 largest MSAs. To remedy this, I chose 11 MSAs diverse with respect to region and poverty concentration history: Chicago, Cleveland, Dallas, Los Angeles, Louisville, New York, Phoenix, Pittsburgh, Providence, Seattle, and Washington, DC. I obtained project addresses in these MSAs through HUD documentation and communication with local housing authorities and reduced the proportion of projects without geographic identifiers to ~5 percent. In total, I assign ~600,000 public housing units to tracts.

Programs other than public housing are not counted in the 1977 data. Therefore, I consider the 1977 time point to capture the concentration of public housing, and the change in assisted housing concentration from 1977 to 2000 represents the transition to dispersal programs—the creation of the Section 8 project-based and voucher programs, LIHTC, and several other small programs with federally subsidized, privately owned units. I likely overestimate the concentration of assisted housing in 1977, since the data are limited to public housing even though about 150,000 Section 8 project-based units and over 100,000 voucher units existed (fewer existed in the 100 largest MSAs) (Schwartz 2010).⁵ In the results section, I discuss how the omission of other programs in the 1977 data may bias results. In 1993, the PSH provides public housing counts but still excludes other housing programs, so I do not link these assisted housing data to 1990 Census data.

The 2000 and 2008 PSH provide the number of units in Census tracts for public housing, vouchers, Section 8 project-based, LIHTC, and other small federally funded programs.⁶ Some vouchers and project-based units were not assigned to tracts in these years. I could not geocode the voucher units that were not assigned to tracts, since street addresses for individual units are not available, but I used the methods described above to reduce missing data for project-based units in the focal MSAs to less than 5 percent. I identify tracts for about 1.4 million project-based units and 1 million vouchers in 2000 and 2008. This time period can be considered a continuation of the focus on dispersal, following the QHWA of 1998.

Poverty Concentration

The dependent variable is poverty concentration in metropolitan areas. Poverty concentration can be measured in many ways to capture different dimensions of concentration (Dwyer 2012; Massey and Denton 1988). I focus on two measures: the proportion of poor residents in a metropolitan area living in neighborhoods with poverty rates over 40 percent (denoted as T , as it relies on a threshold to define high-poverty neighborhoods) and the isolation index (denoted as P^*). T and P^* measure exposure, or the degree to which poor and non-poor residents potentially interact. I focus on exposure measures because the concentrated poverty literature suggests that there are effects for both poor and non-poor residents of increasing exposure to poverty, and deconcentration policies aim to increase assisted residents' exposure to non-poor residents.

T follows from Jargowsky's work (1997, 2003) and is estimated as

$$T = \frac{P_h}{P},$$

where P_b is the number of poor people (those with incomes below the federal poverty threshold) in a metropolitan area living in neighborhoods (Census tracts) with poverty rates over 40 percent and P is the total number of poor people in the metropolitan area. This measure assumes that 40 percent poor is a meaningful threshold, and this threshold has been used in past research as well as policy to identify places where the social problems associated with neighborhood poverty become particularly devastating to residents' well-being (Jargowsky 1997, 2003). Poverty concentration measured in this way increased in the 1970s and 1980s, declined in the 1990s, and increased since 2000 (Jargowsky 1997, 2003; Kneebone, Nadeau, and Berube 2011).

However, Galster (2005) notes that there is no consensus in empirical work on the appropriate poverty threshold associated with serious social problems, with estimates ranging from 5 to over 40 percent. Some MSAs have zero neighborhoods with poverty rates above 40 percent, but if all poor residents lived in neighborhoods in the 30–40 percent poor range, one would still consider poor residents concentrated. Therefore, to create a broader indicator of exposure to poor residents, I calculated the isolation index (P^*). P^* is the summation over all tracts of the tract poverty rate multiplied by the ratio of poor residents in the tract to poor residents in the MSA (Massey and Denton 1988):

$$P^* = \sum_{i=1}^n (x_i/X)(x_i/t_i),$$

where x_i is the number of poor residents in tract i , t_i is the total population of the tract (x_i/t_i is the neighborhood poverty rate), and X is the total number of poor residents in the metropolitan area. P^* can be interpreted as the average poverty rate in a poor resident's neighborhood—the average level of poverty to which a poor person is exposed.

Assisted Housing Concentration

The key independent variable is the concentration of assisted housing in 1977, 2000, and 2008. The PSH data provide counts of assisted housing units in tracts. I measure the concentration of assisted housing units in MSAs using two measures, one assessing exposure and one assessing evenness. First, analogous to the proportion of poor residents in high-poverty neighborhoods (T), I calculate the proportion of assisted housing units in tracts with assisted housing unit (AHU) rates over 10 percent (i.e., more than 10 percent of all housing units in a tract are assisted units). Since no research has examined what a meaningfully high AHU rate is, I chose 10 percent based on the overall distribution of assisted unit rates. I ran analyses with several other thresholds and got similar results, but I acknowledge that this threshold, like the 40 percent poverty threshold, is somewhat arbitrary. I refer to tracts with AHU rates over 10 percent as “high-AHU tracts.” AHU T captures the decline in the reliance on large assisted housing developments and the rise of programs like vouchers that place assisted units in neighborhoods where fewer of the neighbors are also assisted residents.

Second, I measure the concentration of assisted housing through the binary information theory (entropy) index (H), which captures evenness, or the differential distribution of groups across units, rather than exposure (Theil 1972; Theil and Finezza 1971; Massey and Denton 1988). H compares the entropy, or the extent of diversity, of the presence of assisted housing units of the larger metropolitan area to that of the tract. Entropy is calculated as

$$E(u) = u \log_2 \frac{1}{u} + (1 - u) \log_2 \frac{1}{(1 - u)}.$$

To calculate entropy for the MSA, u is the number of assisted housing units in the MSA; to calculate tract entropy, u is the number of assisted housing units in that tract. H is then estimated as the deviation of each tract's entropy ($E_j(u)$) from the metro-area entropy ($E(u)$) as a fraction of the MSA's total entropy, weighted by the total number of housing units:

$$H = 1 - \sum_{j=1}^n \frac{m_j E_j(u)}{ME(u)},$$

where M is the number of housing units in the MSA and m_j is the number of housing units in tract j . H captures how evenly assisted housing units are spread across tracts, a goal of housing programs aimed at deconcentration. H ranges from 0 (no segregation) to 1 (total segregation). H of 0 suggests the AHU rate in all tracts is equal to the overall MSA AHU rate, while H would equal 1 when some tracts had an AHU rate of 100 percent and all other tracts had AHU rates of 0 percent, so no tract had both assisted and non-assisted housing units.

In the appendix, I calculate each measure, T , P^* , and H , for poverty and assisted housing and assess the relationships between all measures. P^* is the most mechanically dependent on population composition, so when there are large compositional changes, P^* reveals more about composition than segregation. For that reason, it is not the best measure for assessing AHU concentration, since the number of assisted units grew substantially during the analysis period. I argue that T and H best capture deconcentration of assisted housing as conceived by policymakers, and T and P^* best capture poverty concentration in terms of its potential impacts on individual well-being.

Analyses

I predict poverty concentration using longitudinal regression with MSA and time-period fixed effects. I pool observations from all three time points, estimating the equation

$$PovCon_{it} = \beta_1 AHUCon_{it} + \beta_k MetroChars_{itk} + \gamma_i + \gamma_t + \varepsilon_{it},$$

where $PovCon$ is poverty concentration (T or P^*) in metro i at time t , $AHUCon$ is AHU concentration (T or H) in metro i at time t , $MetroChars$ is a k -vector of

MSA control variables (described below) in metro i at time t , γ_i represents MSA fixed effects, and γ_t represents time-period fixed effects. *PovCon* and *MetroChars* are measured in 1980, 2000, and 2005–09, while *AHUCon* is measured in 1977, 2000, and 2008. I treat 1977 and 1980 data as time 1, 2000 data as time 2, and 2008 and 2005–09 data as time 3. The MSA fixed effects account for variation across MSAs due to time-invariant variables like region that might affect poverty and AHU concentration. The time fixed effects account for variation within MSAs over time due to time trends that might affect all MSAs, like federal budget cuts. These fixed effects account for considerable variation within and between MSAs, but as with all observational data, there may still be omitted-variable bias if I have failed to control for a characteristic correlated with both poverty and AHU concentration that varies both over time and across MSAs.

I also conducted separate analyses from time 1 to time 2 and time 2 to time 3. The first time period roughly captures the transition from public housing to the use of new programs like vouchers, Section 8 project-based, and LIHTC units. The second time period captures a more explicit focus on dispersal with the continued growth of these programs, continued demolition and redevelopment, and QHWRA provisions. However, the time periods are defined based on available data and do not perfectly correspond to the introduction of various programs, so they are not explicit tests of specific programs. I observe all programs in 2000 and 2008, so the later period measures concentration among the same set of programs and allows me to investigate how the concentration of specific programs is associated with poverty concentration. I estimate AHU concentration as described above separately for (1) vouchers; (2) all project-based units; (3) public housing; (4) LIHTC units; and (5) other project-based units (Section 8 and several other small programs).

I include as control variables MSA characteristics that might influence assisted housing deconcentration and poverty deconcentration. I consider four features in each year: MSA economic characteristics, racial composition, population, and housing market. I control for MSA poverty rate because the exposure measures are somewhat sensitive to changes in the poverty rate and because residential choices may be affected by the MSA poverty rate. I also control for income inequality, which has been shown to impact income segregation and the housing market (Reardon and Bischoff 2011). I account for racial composition with the proportion of residents in the MSA that are non-Hispanic black and the proportion non-Hispanic white as well as racial segregation, measured with the black-white dissimilarity index (the proportion of black residents that would have to move to achieve an even distribution across tracts). I also consider population density to account for crowding conditions that may constrain mobility within a metropolitan area. I consider two housing market characteristics, the proportion of vacant rental units and the proportion of units renting below Fair Market Rate (FMR, as documented by HUD), typically set at the 40th percentile of the area rent distribution and the rental limit for voucher use, as an indicator of the availability of low-rent units.⁷ Finally, I control for the number of assisted units in each metropolitan area.⁸

The Deconcentration of Assisted Housing and the Deconcentration of Poverty

Table 1 presents descriptive statistics for poverty concentration, assisted housing concentration, and the control variables used in regressions for the 100 largest MSAs. Three MSAs without assisted housing units in 1977 are excluded from those estimates. Poverty concentration measured with *T* was fairly stable from 1980 to 2000 (masking a rise in the 1980s and decline in the 1990s), while the average poverty rate in a poor person’s neighborhood (*P**) increased slightly

Table 1: Descriptive Statistics for Poverty Concentration, Assisted Housing Concentration, and Control Variables

Poverty concentration	1980	2000	2005–09
<i>T</i>	0.103 (0.093)	0.101 (0.084)	0.119 (0.085)
<i>P</i> *	0.195 (0.053)	0.199 (0.049)	0.215 (0.047)
Assisted housing concentration	1977	2000	2008
<i>T</i>	0.766 (0.212)	0.521 (0.132)	0.441 (0.148)
<i>H</i>	0.616 (0.117)	0.285 (0.063)	0.219 (0.046)
Control variables	1980	2000	2005–09
MSA poverty rate	0.114 (0.039)	0.115 (0.042)	0.127 (0.039)
Income inequality	0.361 (0.023)	0.401 (0.025)	0.404 (0.022)
% White	0.776 (0.143)	0.677 (0.173)	0.636 (0.176)
% Black	0.120 (0.087)	0.132 (0.091)	0.128 (0.091)
B-W Dissimilarity	0.692 (0.109)	0.600 (0.114)	0.597 (0.100)
Population density	712.804 (1435.200)	870.766 (1560.066)	914.010 (1547.807)
% Rental units below FMR	0.559 (0.114)	0.437 (0.076)	0.489 (0.069)
Rental vacancy rate	0.076 (0.031)	0.073 (0.026)	0.085 (0.027)
Number of AHUs	5,675 (10,574)	23,234 (33,958)	27,436 (41,877)
N of MSAs	97	100	100

Note: Cells present means with standard deviations in parentheses below. MSAs with no assisted units in 1977 (*N* = 3) are excluded.

during this time, consistent with past work (Jargowsky 1997, 2003; Galster 2005). After 2000, poverty concentration increased, on average, among the largest 100 MSAs by both measures, consistent with Kneebone, Nadeau, and Berube (2011).

Both assisted housing concentration indices declined substantially over time, particularly from 1977 to 2000, capturing the shift from public housing to new housing programs aimed at dispersal. Average AHU *T* declined by 25 points from 1977 to 2000 and by 8 points after 2000. The decline in *H* is also striking, from a very high 0.62 (on a scale of 0 to 1) in 1977 to 0.22 in 2008. The average trends in poverty concentration (stability and increase) and assisted housing concentration (decline) appear to be at odds. However, poverty and assisted housing concentration may move in tandem within metropolitan areas, which the regression analyses test.

Table 2 presents results from longitudinal regression models predicting poverty concentration from AHU concentration with MSA time-variant controls and MSA and time fixed effects. These models test whether poverty concentration declined more in MSAs where assisted housing concentration declined more.

As row 1 shows, the proportion of AHUs in high-AHU tracts (*T*) does not significantly predict either measure of poverty concentration. Row 2 indicates a positive and significant relationship between AHU *H* and both measures of poverty concentration. Given that AHU segregation declined in nearly all AHUs, I interpret the positive coefficients in terms of declines in concentration—as assisted housing concentration declined (as it became more evenly dispersed throughout metropolitan areas) from 1977 to 2008, poverty concentration also declined.

The magnitudes are modest: the proportion of poor residents living in high-poverty neighborhoods (*T*) would be about 8 percentage points lower in a

Table 2: 1980 to 2005–09: Longitudinal Regression Models Predicting Poverty Concentration from AHU Concentration in the 100 Largest MSAs

	Poverty <i>T</i>		Poverty <i>P</i> *	
AHU <i>T</i>	0.025 (0.020)		0.008 (0.006)	
AHU <i>H</i>		0.084* (0.037)		0.036** (0.012)
Intercept	–0.080	–0.118	0.001	–0.015
<i>R</i> ²	0.554	0.605	0.765	0.772
MSA FE	Y	Y	Y	Y
Time FE	Y	Y	Y	Y
MSA by Time controls	Y	Y	Y	Y
<i>N</i> (MSA × Time)	297	297	297	297

Note: *N* is 297 observations over 100 MSAs, as 3 MSAs are excluded in 1977 because they do not have assisted housing units at that time. All significance tests are two-tailed.

** $p \leq 0.01$ * $p \leq 0.05$

metropolitan area where AHUs were evenly distributed among neighborhoods compared to a metropolitan area with complete AHU segregation. The average poverty rate in a poor person's neighborhood (P^*) would be nearly 4 points lower in a metropolitan area with completely integrated, compared to completely segregated, AHUs. The actual change in average AHU H from 1977 to 2008 was about 0.4 (table 1), suggesting that assisted housing deconcentration produced declines in the poverty threshold index of about 3 percentage points and in the poverty isolation index of about 1.5 percentage points. The simple simulation described earlier indicated potential direct effects approximately twice as large as these results. Therefore, indirect effects may be offsetting the potential impact of assisted housing deconcentration—poor non-assisted households may replace poor AHUs that leave a high-poverty neighborhood, or non-poor households may move out if AHUs enter their neighborhoods. Moreover, the discrepancy between actual and simulated results emphasizes that even as they are geographically deconcentrated to *more* neighborhoods, AHUs may not be moving to the *lowest-poverty* neighborhoods as in the simulation.

Next, I examined the two time periods separately, and results from 1980 to 2000 were not significant. In 1977, I measure only public housing concentration. Assisted housing deconcentration is the result of both the introduction of new programs and their geographic dispersal, so the 1977 data capture concentration prior to dispersal programs. However, some non-public housing AHUs existed but are not counted in the 1977 data, so AHU concentration is likely upwardly biased, as is the decline in AHU concentration from 1977 to 2000, potentially biasing results. An alternate analysis (not shown) is to consider only the deconcentration of public housing from 1977 to 2000. Neither measure of public housing concentration predicts poverty concentration during this time, despite a decline in public housing H and T . Analyses by program for 2000 to 2008, described below, do show a positive and significant association between public housing concentration and poverty concentration in the later time period. While I cannot rule out bias in the analyses including 1977, I conclude that there is little evidence that the deconcentration of assisted housing from 1977 to 2000 reduced poverty concentration. This is surprising given the large deconcentration of assisted housing during this time, but geographic deconcentration indicates only that AHUs are located in more, not necessarily lower-poverty, neighborhoods.

The 2000 and 2008 data include the same programs, reducing concerns about bias. I examine whether changes in AHU concentration during this time contributed to changes in poverty concentration, and I find a significant, though modest, relationship between poverty concentration and AHU concentration after 2000 (table 3). Poverty concentration increased after 2000 in most MSAs, but AHU concentration continued to decline (AHU H declined in all but 6 MSAs; AHU T declined in all but 14 MSAs). Therefore, poverty concentration may not have declined, but it increased less in places where assisted housing concentration declined.

AHU T significantly and positively predicts poverty concentration T . The effect size is small: a one-point reduction in the percentage of AHUs located in high-AHU neighborhoods reduced the percentage of poor residents living in high-poverty

Table 3: 2000 to 2005–09: Longitudinal Regression Models Predicting Poverty Concentration from AHU Concentration in the 100 Largest MSAs

	Poverty <i>T</i>		Poverty <i>P</i> *	
AHU <i>T</i>	0.099*		0.035**	
	(0.049)		(0.012)	
AHU <i>H</i>		−0.040		0.078**
		(0.104)		(0.024)
Intercept	0.177	0.222	−0.042	−0.033
<i>R</i> ²	0.346	0.219	0.717	0.599
MSA FE	Y	Y	Y	Y
Time FE	Y	Y	Y	Y
MSA by Time controls	Y	Y	Y	Y
<i>N</i> (MSA × Time)	200	200	200	200

Note: All significance tests are two-tailed.

***p* ≤ 0.01 **p* ≤ 0.05

neighborhoods by about 1/10 of a percentage point. The effect may be small because, despite conceptions about the role of public housing in “ghetto” poverty, the mean poverty rate in high-AHU neighborhoods was 24 percent in 2000 and the majority of high-AHU neighborhoods had poverty rates below 40 percent (author’s calculations). High-AHU neighborhoods are therefore not tightly coupled with neighborhoods over 40 percent poor, and a decline in AHU *T* left many of these neighborhoods untouched. AHU *H* after 2000 is not associated with poverty *T*, again emphasizing that many neighborhoods with poverty rates over 40 percent did not have high concentrations of assisted housing. The significant association in the pooled model (table 2) may be driven by the pre-2000 results—from 1977 to 2000, AHU *H* is positively associated with poverty *T* with a *p*-value of 0.105. (The pooled model may attain significance due to increased sample size.) While still not predominant, there were more AHUs in neighborhoods with poverty rates over 40 percent prior to 2000.

Both AHU concentration measures significantly and positively predict poverty isolation after 2000. Row 1 shows that the average poverty rate in a poor person’s neighborhood would be 3.5 percentage points lower in an MSA where no AHUs were in high-AHU tracts compared to one where all AHUs were in high-AHU tracts. Row 2 shows that, comparing an MSA with complete AHU segregation, with some tracts entirely composed of AHUs and others composed of none, to an MSA with no segregation, with the same AHU rate in every tract, the average poverty rate in a poor person’s neighborhood would decline by 8 points. The actual decline in AHU concentration was only 0.07, so the real impact of AHU concentration on poverty concentration was much lower. The decline in poverty concentration is also lower than predicted in the simulation, again indicating barriers to AHUs’ entry into the lowest-poverty neighborhoods and indirect effects on non-assisted households’ mobility.

While the time periods do not perfectly map onto policy changes, two programs may explain why changes in AHU concentration predict changes in poverty concentration more robustly after 2000. First, the demolition and sales of public housing and the focus on mixed-income communities ramped up in the mid- to late 1990s, following the HOPE VI program and viability tests requiring housing authorities to demolish public housing deemed too costly to maintain (Goetz 2011). Second, this finding could be due to the LIHTC program, which is now the largest assisted housing program in the United States, with nearly half of current units added after 2000. Fewer LIHTC units are located in high-poverty neighborhoods and more units are located in low-poverty neighborhoods than in other programs (Schwartz 2010), so the increase in the number of LIHTC units after 2000 may have protected against poverty concentration increases. LIHTC developments may also provide a needed source of reinvestment in a neighborhood (Baum-Snow and Marion 2009), and some past research shows that poverty rates in very high-poverty neighborhoods decline after LIHTC development (Ellen, O'Regan, and Voicu 2009). Finally, the post-2000 period may drive results because a threshold was reached where enough assisted units had been dispersed to lower-poverty tracts to have an impact. The decline in AHU concentration was larger from 1977 to 2000, but perhaps a tipping point was reached only during the continued decline after 2000.

Overall, results indicate that the massive overhaul of assisted housing has only modestly deconcentrated poverty in metropolitan areas, mostly after 2000. This may be due to poor assisted households not moving to neighborhoods with poverty rates low enough to reduce poverty concentration; poor non-assisted residents moving into neighborhoods as poor assisted residents move out; or non-poor residents moving out or avoiding neighborhoods in response to AHU mobility. The deconcentration of assisted housing occurred within a system of neighborhood inequality, and the programs may not have been sufficient to drastically alter the residential patterns of high- and low-income residents given the role of the private market in determining the location of AHUs.

AHU Concentration by Housing Program

The post-2000 period allows for exploration of whether a particular housing program reduced poverty concentration. However, isolating the effects of one program is challenging because the deconcentration of all other AHUs must be taken into account. Further, individual programs may not account for the location of enough units to affect poverty concentration. I estimated AHU H separately for vouchers, all project-based units, and specific types of project-based units: public housing, LIHTC, and other programs.⁹ I repeated the analyses presented in table 3, predicting poverty concentration from AHU concentration by program, entered simultaneously to account for all AHU deconcentration. No individual program's concentration significantly predicted poverty concentration. This may be due to multicollinearity—appendix table 4 presents correlations among the concentration measures of all housing programs, and they are significantly and positively correlated at modest to high levels. Further, the variance inflation factor for the regression model, which indicates how much variance is

inflated due to dependence among the independent variables, suggests that for statistical significance to obtain, the coefficients would have to be between two and four times larger than if there was no multicollinearity. Therefore, I cannot precisely estimate the impact of each housing program while accounting for changes in the others.

Future research should attempt to isolate the impact of each housing program to be most instructive to policymakers. This article focuses on the overall deconcentration of assisted housing, but I present suggestive evidence from models predicting poverty concentration from each housing program separately, without considering other programs, in table 4.

Table 4: 2000 to 2005–09: Longitudinal Regression Models Predicting Poverty Concentration from Housing Program Concentration in the 100 Largest MSAs

	Poverty <i>T</i>	Poverty <i>P</i> *
Voucher <i>H</i>	0.049	0.075*
	(0.124)	(0.030)
Intercept	0.223	– 0.023
<i>R</i> ²	0.233	0.641
All project-based units <i>H</i>	–0.142	0.042^
	(0.099)	(0.024)
Intercept	0.306	–0.058
<i>R</i> ²	0.236	0.563
Public housing <i>H</i>	0.027	0.030*
	(0.055)	(0.013)
Intercept	0.213	–0.036
<i>R</i> ²	0.225	0.588
LIHTC <i>H</i>	–0.020	0.055*
	(0.088)	(0.021)
Intercept	0.235	–0.081
<i>R</i> ²	0.214	0.653
Other project-based units <i>H</i>	–0.004	0.035
	(0.036)	(0.024)
Intercept	0.218	–0.074
<i>R</i> ²	0.221	0.537
<i>In all models</i>		
MSA FE	Y	Y
Time FE	Y	Y
MSA by Time controls	Y	Y
<i>N</i> (MSA × Time)	200	200

Note: Concentration of each program was included in separate longitudinal regression models predicting poverty concentration. All significance tests are two-tailed.

* $p \leq 0.05$ ^ $p \leq 0.10$

No single program predicts the threshold measure of poverty concentration. This may be because, as described above, AHUs are not as prevalent in these neighborhoods as often assumed, or because individual programs do not have the sheer numbers in these neighborhoods. The concentration of each program except other project-based programs significantly or borderline significantly predicts the poverty isolation index. The largest effect size of any program is for voucher units—the average poverty rate in a poor person's neighborhood would be nearly 8 points lower in a metropolitan area where vouchers are evenly distributed among tracts compared to a metropolitan area where some neighborhoods have no vouchers and others consist entirely of voucher units. These results must be considered biased, as they do not account for other AHU programs, but they suggest that both voucher and project-based programs are associated with poverty deconcentration.

Discussion

Since its inception, assisted housing has been intertwined with racial and economic segregation and inequality among neighborhoods. Despite the promise of policy interventions in reducing neighborhood inequality, assisted housing has at times had the opposite result. Hirsch (1983) argued that while the construction of public housing in former slum areas in the first half of the 20th century was aimed at improving the living conditions of poor inner-city residents, it eventually reinforced the structural inequalities it was trying to eradicate, creating “the second ghetto.” Assisted housing programs since the 1970s have focused on deconcentrating assisted housing, motivated in part by the hope of reducing racial and economic segregation (and undoing any harm that public housing did in perpetuating these inequalities). This policy shift had the potential to reduce poverty concentration by facilitating some poor residents' mobility to lower-poverty neighborhoods. While many assisted residents are now exposed to somewhat more advantaged neighborhoods, my findings show that overall, the broad shift in housing policy from 1977 to 2008 only modestly reduced poverty concentration in metropolitan areas, despite the drastically altered geographic landscape of assisted housing. Future research should continue to explore effects on racial segregation, but my results do not foretell large impacts.

The deconcentration paradigm in assisted housing policy focuses on the dispersal of assisted units, with increasing focus over the past 20 years on dispersal to low-poverty neighborhoods. Arguably, HUD has not undertaken a systematic policy to reduce poverty concentration in metropolitan areas, but poverty deconcentration could have occurred through the dramatic deconcentration of assisted housing, one of the few policies facilitating the mobility of low-income residents. However, many other structural forces cluster poor residents in neighborhoods, including long-standing patterns of racial and economic segregation, limited economic opportunities for low-skilled workers, and zoning and development policies. These factors have led to a durable system of neighborhood inequality, and the enduring place hierarchy may limit assisted housing's ability to reduce poverty concentration by shaping both where assisted housing is located and its effect on the residential choices of non-assisted residents. The state's intervention

in the housing market relies on the private market to determine the location of assisted units, and as Logan and Molotch (1987, 170) foretold, “use value goals like ... integration ... cannot be shaped by a national housing policy in which government passively writes checks to be spent in the marketplace.” Despite this warning, policymakers and many social scientists did see housing policy as a tool to break up concentrated poverty, but my results indicate limited success, supporting Logan and Molotch’s supposition.

Though modest in magnitude, the relationship between assisted housing deconcentration and poverty deconcentration is significant and positive, particularly after 2000, suggesting that assisted housing became increasingly important in facilitating poverty deconcentration during a time when neighborhood poverty rates and poverty concentration increased. As economic segregation increased, distributing assisted units across more neighborhoods in metropolitan areas may have provided critical affordable housing opportunities for low-income families in lower-poverty neighborhoods, tempering rises in poverty concentration. This emphasizes the need for housing programs to produce affordable housing in lower-poverty neighborhoods, rather than just providing rental subsidies through vouchers. One program that produces affordable rental housing is the LIHTC program, which may explain why AHU concentration better predicts poverty concentration after 2000. Future research should examine the differing roles of various housing programs in facilitating or tempering the deconcentration of poverty. The descriptive results presented here indicate that both tenant- and project-based programs may be effective.

For assisted housing policy to successfully deconcentrate poverty, programs must facilitate low-income residents’ mobility to stably low-poverty places and retain or attract non-poor residents to areas where assisted housing is located. Current policies must do more to place assisted residents in low-poverty communities. Voucher users may be priced out of these communities, and landlords in competitive markets have little incentive to rent to voucher users or be subject to HUD inspections (DeLuca, Garboden, and Rosenblatt 2013). Section 8 project owners may not renew their contracts if they can get higher rents on the private market (Khadduri and Wilkins 2008). Housing policies must incentivize the production of affordable housing and landlord participation in low-poverty neighborhoods, perhaps through property tax rebates for landlords who accept vouchers or by modifying FMR to increase vouchers’ value in high-cost areas. Zoning laws and local affordable housing initiatives are also critical in guaranteeing a supply of assisted housing in low-poverty neighborhoods (Katz and Turner 2008). Policies must also facilitate assisted residents’ movement to stably lower-poverty neighborhoods through mobility counseling that informs families about employment, schooling, and transportation opportunities and provides long-term case management. Analyses of experimental mobility programs find little evidence that assisted residents do not want to live in higher-SES neighborhoods (Edin, DeLuca, and Owens 2012), so understanding the barriers is critically important. Public and private investments in the commercial and residential amenities of high-poverty neighborhoods is also critical, including after public housing demolition (though there is little consensus on the most effective place-based policies [DeLuca 2012]).

Assisted housing units have been geographically dispersed into moderately lower-poverty neighborhoods, and individual assisted residents may experience benefits to mental and physical health and feel safer living in these slightly improved neighborhood contexts, as evidence from the MTO experiment suggests (Briggs, Popkin, and Goering 2010). However, residents, both assisted and non-assisted, poor and non-poor, remain at risk for the deleterious effects associated with living in very high-poverty neighborhoods. Assisted housing policy is arguably the dominant urban antipoverty policy since the 1990s (Goetz 2003), but my results suggest that even large policy interventions have not been enough to dramatically deconcentrate poverty in the face of durable inequality between neighborhoods. Of course, assisted housing is not an entitlement program, and it provides subsidies to only a minority of poor residents. The majority of low-income residents do not live in assisted housing, and they remain subject to vast inequalities between neighborhoods. Perhaps the most promising way housing policy can deconcentrate poverty is to intervene in the housing market to guarantee affordable housing for all in stably lower-poverty neighborhoods. Zoning laws, production of affordable housing, and investment in inner-city neighborhoods benefit poor residents regardless of whether they live in assisted housing. Finally, others posit that the spatial focus of the poverty deconcentration paradigm ignores the underlying issue of poverty (Crump 2002), arguing that the most effective policies would intervene in the labor market as well as the housing market.

Notes

1. Assisted housing denotes means-tested federally funded housing programs supported by both project- and tenant-based subsidies. I refer to residents living in assisted housing as “assisted residents” or “assisted households.”
2. In 1977, approximately 10.5 million households were poor and about 1.1 million assisted housing units existed (US Census Bureau 1979; HUD 1977). In 2008, there were approximately 18.8 million poor households and nearly 4.5 million assisted housing units (US Census Bureau 2009a; HUD 2008).
3. The ACS data aggregate surveys over five years and have a smaller sampling frame, which results in potential comparability issues with Census data, but they are the only post-2000 socioeconomic data available at the tract level. I use measures considered comparable with Census data (US Census Bureau 2009b).
4. Eighteen MSAs have Primary Metropolitan Statistical Areas (PMSAs) delineating major urban areas inside them. For these, I used the PMSA as the delineation of the metropolitan area.
5. The 1977 data provide the latest report from 1971 to 1977, and a third of the data were collected prior to 1977, so the number of non-public housing units in existence is less than the 250,000 estimate.
6. Data include mortgage subsidy programs (Section 236 and 221(d)3) and housing for disabled (Section 202) and elderly residents (Section 811). Data exclude the USDA’s rural housing program (Section 515) as well as Indian Housing, and the HOME and CDBG programs. These data include rental units as well as federally funded homeownership programs, though the vast majority of assisted residents are renters.
7. HUD provides FMR data for MSAs since 1983. I use 1983 FMR data for time 1, 2000 FMR data for time 2, and 2008 FMR data for time 3. FMR is based on the number

of bedrooms, so I merged FMR data with Census data on rent distributions by bedroom number. Then, I estimated the proportion of rental units rented below FMR. Estimates are imprecise, as Census rent categories vary across years and the rent categories do not perfectly match up to FMR limits.

8. I tested both AHU rate and raw number of AHUs in models. The coefficient for AHU concentration does not substantially vary by which measure is used.
9. I also estimated T by housing program, but T does not significantly predict either measure of poverty concentration, perhaps because few tracts are composed of over 10 percent of any single housing program.

About the Author

Ann Owens is an Assistant Professor of Sociology and, by courtesy, Spatial Sciences at the University of Southern California. Her research interests include urban sociology, sociology of education, social stratification, and social policy. Current research projects include further research on the implications of assisted housing policy for neighborhood inequality; the role of assisted housing in intergenerational economic and neighborhood mobility; and an examination of the causes and consequences of economic school segregation.

Supplementary Material

Supplementary material is available at *Social Forces* online, <http://sf.oxford-journals.org/>.

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What Do Unions Do? A Cross-National Reexamination of the Relationship between Unionization and Job Satisfaction

Lena Hipp, *WZB Berlin Social Research Center*
 Rebecca Kolins Givan, *Rutgers University*

What is the relationship between unionization and job satisfaction? Despite a great deal of research over several decades, the answer to this question is still uncertain. In contrast to earlier work, which analyzed mostly data from individual companies or countries, we examine the association between union membership and job satisfaction in a cross-national perspective. We therefore combine large-scale survey data with country-level information about union and labor-market characteristics. Our multilevel approach allows us to examine whether and why the unionization–job satisfaction relationship varies across countries. The main finding of our analyses is that the relationship between union membership and job satisfaction varies across countries and that unions matter only for certain aspects of job satisfaction—those that can more readily be changed by unions. This effect, moreover, is contingent on countries' industrial relations systems, in particular union density, bargaining coverage, and the centralization of bargaining agreements. Taken together, our results show that in order to understand how unionization influences job satisfaction, it is important to distinguish between the various aspects of job satisfaction and to consider the larger context in which unions operate.

For over 200 years ... economists and other social scientist, labor unionists, and businessmen and women have debated the social effects of unionism. Despite the long debate, however, no agreed-upon answer has emerged to the question: What do unions do? (Freeman and Medoff 1984, 3)

In their seminal book *What Do Unions Do?* Freeman and Medoff (1984) systematically examined the impact of unions in the United States on various economic, behavioral, and attitudinal outcomes, including workers' job satisfaction. Their study is a prominent and influential example of an entire line of research that

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found unionized workers to be less satisfied with their jobs than their nonunionized colleagues (e.g., Borjas 1979; Freeman 1978; Kochan and Helfman 1981). More recent work, however, has challenged the paradoxical conclusion that unions are actually bad for workers' job satisfaction (Gomez-Mejia and Balkin 1984; Meng 1990; Pfeffer and Davis-Blake 1990; Gordon and Denisi 1995; Bryson, Cappellari, and Lucifora 2004, 2010; Powdthavee 2011; Donegani and McKay 2012). Hence, despite many years of research, the relationship between unionization and job satisfaction remains unclear.

Another body of literature showed that what unions *can* actually do strongly depends on the larger environment in which they operate (Blanchflower and Freeman 1992; Wallerstein 1999; Wallerstein, Golden, and Lange 1997; Katz and Darbishire 2000; Baccaro and Howell 2011). These studies examined cross-national differences in unionism and union outcomes and were particularly interested in the institutions that enable and constrain union efforts to improve working conditions, such as union density, labor and employment regulations, collective bargaining structures, and welfare state configurations. The context in which unions operate should therefore—either directly or indirectly—influence workers' attitudes and perceptions, including their job satisfaction.

By connecting the comparative research on cross-national differences in unionism with the research on subjective work-related outcomes, we reexamine the relationship between union membership and job satisfaction across countries, thus bridging two literatures that have thus far been disconnected. Based on the findings of these two streams of research, we expect both job satisfaction levels and the relationship between unionization and job satisfaction to be contingent on cross-national institutional differences. We thus examine how macro-level institutions and union membership interact in order to better understand the relationship between union membership and job satisfaction. In other words, *does the relationship between unionization and job satisfaction differ across countries, and if yes, how can we explain these differences?*

This paper is organized as follows: we first review the existing literature on the relationship between unionization and job satisfaction. Next, we link these findings to previous research on political-economic systems to explain how institutional differences at the national level have the potential to affect workers' job satisfaction. To test the posited relationships, we apply multilevel modeling techniques to a data set that we created by combining individual-level information on employees from the International Social Survey Program with national-level information from various data sources on unions, labor-market regulations, and economic conditions. In the last section, we discuss the results and limitations of our analyses.

The Unionization–Job Satisfaction Relationship

For a long time, the debate on unions has been dominated by the narrative that unions are bad for workers' job satisfaction (Borjas 1979; Freeman 1978; Freeman and Medoff 1984; Bender and Sloane 1997; Clark 2001; Garcia-Serrano 2009; Green and Heywood 2010; Hersch and Stone 1990; Heywood,

Siebert, and Wei 2002; Kalleberg 1977; Kochan and Helfman 1981; Miller 1990; Odewahn and Petty 1980). This is an apparent paradox, given that unions essentially seek to improve workers' wages, their job security, and their working conditions by providing mutual aid, advocating for the enactment of labor-friendly legal and political change, and negotiating collective bargaining agreements (e.g., Webb and Webb 1926; Hyman 1997; Givan 2007; Rosenfeld 2014).

One way to explain the counterintuitive finding that unionization decreases workers' job satisfaction is that union members are not necessarily less satisfied with their jobs than nonunion members, but that they have the means to complain without fearing that this will be held against them. They can more comfortably express their discontent because they have access to grievance procedures and collective representation, and because collective bargaining agreements give them stronger dismissal protection. This "voice" hypothesis was originally suggested by Freeman (1978) and has been widely embraced ever since (e.g., Hammer and Avgar 2005; Hersch and Stone 1990). Some proponents of this hypothesis also argue that union members may become politicized by being a member of a union (Ross [1953] even talks about "manufactured discontent"), and thus have greater expectations about the quality of their jobs than their nonunion counterparts (Berger, Olson, and Boudreau 1983; Barling, Fullagar, and Kelloway 1992), which would make them more likely to express dissatisfaction (Freeman and Medoff 1984).

The second explanation refers to the fact that unions have a limited capacity to influence individuals' job satisfaction. The degree to which workers have control over their jobs is an important determinant of satisfaction, but as a result of the bargaining process, unions cede key aspects of job control (Kalleberg 1977). Because union members are more likely than nonunion members to work in jobs with narrow job classifications and restrictive work rules, they may therefore also be less happy with their jobs (Cranny, Smith, and Stone 1992; Hackman and Oldham 1976; Kalleberg 1977). By contrast, unions have a positive influence on the "bread and butter" aspects of work, such as wages or job security (e.g., Kochan and Helfman 1981; Meng 1990; Gomez-Mejia and Balkin 1984; Artz 2012). However, due to union members' "flatter earning profiles," the positive effects unions have on wages and benefits may diminish over time (Borjas 1979). Hence, when job satisfaction is broken down into satisfaction with narrower aspects of the job, the results tend to become more complex.

A third type of explanation refers to the misspecification of statistical models based on survey. For one, survey studies may generate misleading results because they ignore important covariates. For example, unionization is more likely to occur within industries and occupations with poor working conditions, which explains why union members tend to be less satisfied with their jobs than their nonunionized counterparts (Locke 1969, 1976; Hoppock 1935; Pfeffer and Davis-Blake 1990; Renaud 2002; Gordon and Denisi 1995; Reynolds and Brady 2012). In addition, survey studies may not account for the possibility of reverse causality. It is possible that rather than having low job satisfaction due to being involved in a union, workers may join a union or take a unionized job because they are unhappy with their work. Union members may thus have lower levels of

job satisfaction because they are more frustrated and less satisfied in general, rather than because they are union members. This explanation dovetails with the findings of those studies that—after controlling for job characteristics and endogenous selection—do not find any differences in job satisfaction between union members and nonunion members (e.g., Bryson, Cappellari, and Lucifora [2004] in their analyses of British data).

The final type of explanation also emphasizes the working conditions of unionized workers and refers to the role of contextual factors and the larger environment in which unions operate. Here, the suggestion is that job satisfaction is not actually influenced by unionization itself, but by other factors that can easily be confounded with unionization, such as coverage by collective bargaining agreements and the industrial relations climate. Collective bargaining coverage simply refers to the percentage of the workforce that is working under a collective bargaining agreement; the industrial relations climate is the aggregate degree of strife or collaboration between labor and management. Some studies refer to collective bargaining systems and argue that higher levels of job satisfaction among nonmembers and free riders, and not lower job satisfaction among members, explain the negative association between unionization and job satisfaction (Odewahn and Petty 1980; Powdthavee 2011; Clark 2001). Other studies argue that union members express lower levels of job satisfaction due to the conflict-laden nature of the bargaining relationship (Gordon and Denisi 1995; Kochan and Helfman 1981; Freeman and Rogers 1999; Bender and Sloane 1997; Garcia-Serrano 2009).

In summary, the finding of a negative relationship between unionization and job satisfaction does not stand up to scrutiny once appropriate controls are included and important contextual factors are considered. A number of studies even found a positive relationship between union membership and job satisfaction (Gomez-Mejia and Balkin 1984; Meng 1990; Pfeffer and Davis-Blake 1990; Powdthavee 2011) or no relationship at all (Bryson, Cappellari, and Lucifora 2004, 2010; Gordon and Denisi 1995). Hence, the “association between union membership and job satisfaction is quite country-specific,” as Donegani and McKay (2012, 481) conclude based on their analyses of data from 18 European countries.

As most studies of the union–job satisfaction relationship have been conducted as single-country studies, particularly in the English-speaking world, this paper seeks to delve more deeply into the issue of how contextual factors, particularly national-level institutions and union characteristics, may affect the association between union membership and job satisfaction. By providing a unique cross-country perspective, it closes an important gap in the literature and links the micro-oriented literature on job satisfaction with macro-level studies on cross-country variation.

Cross-National Variations in Industrial Relations Institutions

Most of the comparative literature on industrial relations systems has shown that unions perform different roles in different countries depending on the institutional and regulatory environment (Hall and Soskice 2001; Katz and Darbishire 2000;

Thelen 2001; Greer and Hauptmeier 2008; Frege 2007; Frege and Kelly 2003, 2004; Doellgast, Batt, and Sørensen 2009; Doellgast, Holtgrewe, and Deery 2009; Streeck 1992; Givan and Hipp 2012; Reynolds and Brady 2012; Baccaro and Simoni 2008). Despite some convergence in industrial relations institutions across countries and the decline of both union membership and collective bargaining, these institutions retain some key historically determined national characteristics (Hall and Thelen 2009; Howell and Givan 2011; Piore and Safford 2006). Based on the growing number of multilevel studies showing that country-level characteristics are a major explanation for variations in individual-level outcomes (e.g., Jones and Smith 2001; Sjöberg 2010; Weldon 2006), we indeed have reason to believe that the relationship between unionization and job satisfaction varies across countries, and that such variation can be attributed to differences in important union characteristics. Cross-country differences in union density, bargaining coverage, centralization of bargaining systems, and the industrial relations climate should therefore also affect individuals' job satisfaction, particularly differences in job satisfaction between unionized and nonunionized workers.

Union density is one important indicator of the structural differences between unions in various countries. Despite an overall decline in membership, it still varies considerably across countries (Blanchflower and Bryson 2004; Ebbinghaus and Visser 2000). The Scandinavian countries and Belgium, for example, are characterized by high union density because trade unions offer a voluntary scheme for unemployment benefits (also known as the "Ghent System"). This system provides a strong incentive for workers to join a union and helps organized labor in these countries to attract and retain members during downturns and resist downward pressure on wages (Scruggs and Lange 2002; Rothstein 1992). In France, in contrast, trade union density is very low, but French unions are nonetheless influential, since the terms of industry-wide and regional bargaining agreements can also be extended to nonunion workers (Howell 2005). In the United Kingdom and the right-to-work states of the American South, low union density is actually an indicator of low union strength. Therefore, it is unlikely that—or at least unclear how—union density would influence workers' job satisfaction in some generalized way.

Bargaining coverage, which is another important indicator of union strength, should actually be more likely to influence workers' job satisfaction than union density. Bargaining coverage indicates the degree to which an individual's terms and conditions of employment are affected by collective agreements. It is defined as the percentage of the workforce whose terms of employment and wages are covered by a collective bargaining agreement (regardless of whether the individual is a member of a union). In 2005, the year analyzed in this paper, bargaining coverage in the United States, for example, was only 13.7 percent (OECD 2012). This low coverage rate is due to the fact that the contracts negotiated by local unions only apply to a limited number of workers. Likewise, bargaining coverage was also low in Japan (16.4 percent) (*ibid.*). In contrast, bargaining coverage in the corporatist countries, particularly in those who have adopted the "Ghent System" (Belgium and Scandinavia, but also France, Spain, the Netherlands, and Cyprus), was well above 80 percent (*ibid.*). Here, unions bargain with employer

organizations for national agreements; these wage settlements are then linked to economic policies at the national level, which result in relatively equal income distributions (Card 2001; Rueda and Pontusson 2000; Blanchflower and Bryson 2004; Katz and Darbishire 2000). The degree to which collective bargaining agreements also cover nonunionized employees may thus positively affect job satisfaction—for both unionized as well as nonunionized workers, as the literature on free riding suggests (e.g., Powdthavee 2011).

The level at which collective bargaining agreements are negotiated is another important characteristic distinguishing countries' industrial relations systems. Bargaining occurs at a number of different levels: at the workplace level, the company level, between multiple employers within an industry, or at the sectoral or national level. The degree to which economy-wide objectives such as wage compression or wage restraint are considered in bargaining agreements is an important characteristic distinguishing these different systems. Most extant research on bargaining centralization and coordination therefore focuses on wage dispersion or economic inequality (Moene and Wallerstein 1997; Oliver 2011; Wallerstein 1999; Baccaro and Simoni 2010). Wallerstein (1999), for example, found that more centralized wage determination—which may include mechanisms other than traditional collective bargaining—compresses the national wage distribution. In other words, higher levels of collective bargaining lead to lower levels of wage inequality. While we can triangulate this finding by examining other work on the relationship between wage inequality and job satisfaction (Clark, Kristensen, and Westergård-Nielsen 2009; Pfeffer and Davis-Blake 1990), scholars have not, to date, examined the relationship between the collective bargaining level and job satisfaction. Nonetheless, based on these theoretical considerations, higher levels of bargaining should be positively associated with union members' job satisfaction.

The industrial relations climate, that is, the quality of labor-management relations within a country or an organization, is another potential factor contributing to union members' job satisfaction. When unions and employees are integrated into the decision-making processes and management does not or cannot exert unilateral power over the labor process, cooperative and friendly labor-management relationships are more likely (Belman 1992; Katz, Kochan, and Gobeille 1983). The degree to which labor-management relations are cooperative (as is the case in Denmark, for example) or confrontational (as is the case in France, for example) depends on several factors: union ideology, joint decision-making processes between management and labor, and unions' means of ensuring positive outcomes for their members (Kochan and Osterman 1994; Kochan, Katz, and McKersie 1994). Cooperative union-management relations have been shown to have a positive impact on firm performance—particularly on staff turnover rates and absenteeism (Deery, Iverson, and Erwin 1999; Deery and Iverson 2005). Proceeding from the idea that unions help workers articulate their discontent, a cooperative industrial relations climate could be the result of satisfied workers but could also contribute to workers' job satisfaction. This suggests that a more cooperative industrial climate will be positively associated with workers' job satisfaction.

In summary, what unions do and how specific attributes affect workers' job satisfaction varies quite a bit between countries. This paper's driving assumption

is that institutional differences between unions affect what union members (as well as nonunion members) think and feel about their jobs. In other words, we expect that unions in various countries will have distinct direct and indirect effects on workers' job satisfaction. Differences in unionism should therefore be another explanation for why the findings on the association between union membership and job satisfaction are not straightforward and why this association varies between countries.

Methodology

To answer the question of whether the association between union membership and job satisfaction varies across countries and whether differences in these associations can be explained by differences in unionism, we analyze individual-level survey data from the International Social Survey Program (ISSP). The ISSP is an ongoing, collaborative survey of a wide array of countries, with annually changing survey topics. The 2005 wave, which we use for our analyses, was dedicated to individuals' "work orientations." For our analyses, we combined these data with country-level information on industrial relations systems and economic information from the Organisation of Economic Cooperation and Development (OECD); the International Labour Organization (ILO); the Database on Institutional Characteristics of Trade Unions, Wage Setting, State Intervention and Social Pacts (ICTWSS); and the Ifo Institute for Economic Research (CESifo).

Job satisfaction is generally defined as the fulfillment of one's wishes, expectations, or needs. It is "a pleasurable or positive emotional state resulting from the appraisal of one's job or job experiences" (Locke 1976, 1304). Whereas economists and sociologists tend to use single-item measures of job satisfaction (Bryson, Cappellari, and Lucifora 2004; Sousa-Poza and Sousa-Poza 2000), psychologists favor measures based on multiple dimensions (Wanous et al. 1997). In our study, we use both types of measures. In addition to the single-item question on job satisfaction provided in the ISSP ("How satisfied are you in your (main) job?"), we also use two composite measures of job satisfaction—one capturing workers' satisfaction with the material aspects of their jobs and the other capturing workers' satisfaction with the content of their jobs.

These measures were constructed by calculating the discrepancy between *how much there should be* and *how much there is* for a total of seven job dimensions (see Porter [1961] or Kalleberg [1974] as two early proponents of such an approach, and Jiang and Klein [2002] for a more recent illustration). For example, respondents' satisfaction with job security was based on the questions of (1) how important job security was for them personally; and (2) how secure their jobs actually were. In total, respondents were asked these two types of questions on seven dimensions typically used in studies of job satisfaction (Locke 1976; Brooke, Russell, and Price 1988; Clark 2005): income, opportunities for advancement, job security, usefulness of the job, helpfulness of the job, interestingness of the job, and job autonomy (the exact wording of all questions can be found in appendix A). The difference in respondents' answers to the two sets of questions—both of which use a five-point Likert-type answering scale—was used to calculate

Table 1. Coding Scheme Job Satisfaction

Expectations regarding job characteristics ^b	Actual job characteristics ^a				
	Strongly agree (5)	Agree (4)	Neither nor (3)	Disagree (2)	Strongly disagree (1)
Very important (5)	5	4	3	2	1
Important (4)	5	5	4	3	2
Neither nor (3)	5	5	5	4	3
Not important (2)	5	5	5	5	4
Not important at all (1)	5	5	5	5	5

Note: Higher value indicate higher satisfaction; that is, 1 means that an individual is very dissatisfied with a certain aspect of her or his job, and 5 means that an individual is very satisfied with a certain aspect of her or his job.

^aExample: My job is interesting.

^bExample: How important is an interesting job?

individuals’ job satisfaction with each of these dimensions. The coding scheme is provided in table 1.

As can be seen in table 1, individuals whose needs and expectations regarding particular job dimensions are fulfilled or even over-fulfilled—that is, the difference between the situation as “it should be” and as “it is” is ≥ 0 —are coded as being the most satisfied with this particular dimension. Those who have great needs but whose jobs do not provide outcomes that match these needs at all are coded as being the least satisfied—that is, the difference between the situation as “it should be” and as “it is” is -4 . For example, if individuals say that having a high income is “very important” to them (highest out of five possible answer categories) but also say that the pay at their current job is “not high at all” (lowest out of five possible answer categories), they are coded as being “very dissatisfied with their income.” In contrast, those who say that income is “very high” are coded as being “very satisfied with their income” regardless of the importance they attribute to a high income, as their needs and expectations are fulfilled in any case.¹

These seven newly created items of job satisfaction were then combined into one single measure. Based on an exploratory factor analysis (provided in appendix B), we extracted two factor scores, which we use as our dependent variables. The first is *satisfaction with the material aspects of the job*, which is based on individuals’ satisfaction with income, opportunities for advancement, and job security. The second is *satisfaction with the job content*, which is based on individuals’ satisfaction with four aspects: how useful their job is, how helpful it is, how interesting it is, and their job autonomy. The Cronbach’s alphas for both measures averaged across countries are 0.69 (satisfaction with the job’s material aspects) and 0.65 (satisfaction with the job’s content), and are thus slightly below the recommended level of 0.70 (Nunnally 1978). This is because Cronbach’s alpha is affected by the number of items integrated in the scale, and because the items are not equally internally consistent across all countries (see Bentley et al. 2013; Schumann 2009).

The distinction between these two dimensions is beneficial for our analyses because it also allows us to differentiate between the facets that can actually be influenced by unions—that is, individuals' material satisfaction with their jobs—and those that are unlikely to be influenced by unions—that is, satisfaction with the content of the job. Both single-item measures and scales that incorporate several measures—such as the commonly used Brief Index of Affective Job Satisfaction (Thompson and Phua 2012), Job Descriptive Index (Smith, Kendall, and Hulin 1969), or Minnesota Satisfaction Questionnaire (Weiss et al. 1967)—combine these dimensions and may therefore be unable to adequately capture the unionization–job satisfaction relationship. A brief examination of the correlation between our three measures shows that it does indeed make sense to distinguish between different facets of job security, as the three measures are only weakly correlated with one another (around 0.3).

Our main explanatory variables at the country level are (1) union density, which is the proportion of unionized individuals in the total workforce; (2) the dominant level at which wage bargaining takes place (5 = national or central level; 1 = local or company bargaining); (3) the industrial relations climate (1 = generally confrontational; 7 = generally cooperative);² and (4) bargaining coverage, which is the proportion of all wage and salary earners in employment with the right to bargain that are covered by wage bargaining agreements.³ Table 2 provides an overview of all country-level variables and their respective source (including the country-level control variables).

Our main explanatory variable at the individual level is union membership. In addition, we include a number of control variables to account for alternative explanations. As important demographic and workplace characteristics, we include age, gender, relationship status, the presence of children in the household, and education. In addition, we also adjust our models for part-time work, supervisory position, occupation, and public sector employment (scholars have found that public sector employment, which generally comes with more job security and more generous benefits, is associated with higher job satisfaction than private sector employment; see, e.g., Fasang, Geerdes, and Schömann [2012] for an international comparison of job satisfaction). The occupational categories (based on the ISCO88 categories) are a proxy for skill level and job mobility, with the assumption that higher levels of both are associated with greater job satisfaction.

Since the previous literature has indicated that working conditions are of central importance in correctly specifying the relationship between unionization and job satisfaction (e.g., Gordon and Denisi 1995; Pfeffer and Davis-Blake 1990; Renaud 2002), our models are also adjusted for the degree to which a job is perceived to be exhausting, dangerous, and stressful, as well as for the degree of physical work a job requires. As additional indicators of unobserved heterogeneity, our models also include self-assessed work–life balance and recent participation in training courses. These variables indicate higher levels of job control and tend to be associated with higher overall job satisfaction. Self-reported relationships with both management and colleagues are also important, as we know these relationships affect general job satisfaction (see, for example, Bateman and Organ [1983]). Therefore, these variables are also included in our models.

Table 2. Overview on Country-Level Union, Policy, and Economic Characteristics

Country	Union density	Bargaining coverage	Coordination level	Industrial relations climate	Unemployment rate	Dismissal protection
Australia	22.1	60	2	4.6	5.1	17
Belgium/ Flanders	52.9	96	3	4.1	8.4	27
Bulgaria	21.3	25*	3	4	10.1	29
Canada	27.7	32	1	4.6	6.8	4
Czech Republic	21.5	44*	2	4.8	7.9	27
Denmark	71.7	82	2	6.1	4.8	10
France	7.8	95	2	3.3	8.8	56
Germany	21.6	63	2	4.8	11.1	44
Hungary	17.5	35	2	5	7.2	34
Japan	18.8	16.4	1	6.1	4.4	17
Latvia	16.1	20*	1	4.5	8.7	49
Netherlands	21	82	4	5.4	5.2	42
Norway	54.9	72	3	5.6	4.6	41
Portugal	21.2	62*	2	4.5	7.6	48
Spain	15	81	3	4.6	9.2	56
Sweden	76.5	92	3	5.3	7.7	39
Switzerland	19.4	48	2	6	4.4	23
United Kingdom	28.4	35.3	1	5.3	4.8	7
United States	12	13.7	1	5.2	5.1	0
Data source	ISSP	ICTWSS	ICTWSS	CESifo	World Bank	World Bank

Note: All data refer to the year 2005. Exceptions are marked with an asterisk (see note 4 for more details).

Since the factor score on satisfaction with the material aspects of a job also includes job security, we also include the strength of dismissal protection (World Bank Database 2012)⁴ as a country-level control variable in our analyses. The models are, moreover, adjusted for country-specific unemployment rates (World Bank Database 2012) because unemployment has been shown to be positively related to job satisfaction (e.g., Shikiar and Freudenberg 1982).

By restricting our analyses to employees working at least part-time and to those countries for which macro-level indicators were available, our final sample yields a total of 11,234 observations in 20 countries (in the descriptive analyses, the total sample includes 14,669 in 26 countries). The minimal sample size per country is 383, and the maximal sample size per country is 947. Observations with missing variables were deleted. No variable used in the analyses has more

than 5 percent missing data. The 20 countries that are included in our multilevel analyses are Australia, Belgium (Flanders), Bulgaria, Canada, the Czech Republic, Denmark, Finland, France, Germany, Hungary, Japan, Latvia, the Netherlands,⁵ Norway, Portugal, Spain, Sweden, Switzerland, the United Kingdom, and the United States. Due to missing macro-level data, Cyprus, Finland, Ireland, New Zealand, South Africa, South Korea, and Taiwan are included in only our single country analyses.

To examine the relationship between union membership and job satisfaction in a comparative perspective, we proceed with the following steps in our analyses. We first provide simple descriptive findings on unionized and nonunionized workers' job satisfaction. In the second step, we examine whether differences in union members' and nonunion members' job satisfaction persist once other explanations are taken into account by running separate regressions for each country in our study sample. In the third step, we examine how national-level union characteristics influence the relationship between unions and job satisfaction by jointly analyzing the individual- and country-level data in a linear random slope model.⁶ We apply multilevel modeling techniques to take the "nestedness" of the data into account (Rabe-Hesketh and Skrondal 2005). Our model is specified as follows:

$$Y_{ij} = \gamma_{00} + \sum \gamma_{k0} \times X_{ij} + \sum \gamma_{0k} \times Z_{.j} + \sum \gamma_{kk} \times X_{ij} \times Z_{.j} + u_{0j} + n_{kj} \times X_{ij} + \varepsilon_{ij},$$

where

Y_{ij} is the job satisfaction of individual i in country j

X_{ij} are the individual-level characteristics for the individual i in country j

$Z_{.j}$ are the country-level characteristics for country j

γ_{00} is the overall intercept

γ_{k0} are the fixed individual-level parameters

γ_{0k} are the fixed country-level parameters

γ_{kk} are the cross-level interaction parameters

u_{0j} is the country-specific deviation from the overall intercept

n_{kj} is the random parameter for union membership

ε_{ij} is the residual for the i th individual in country j .

Robustness Checks

In order to assess the robustness of the results presented below, we ran a couple of sensitivity tests. To ensure the validity of our model specifications, we examined whether the subjective measures used as control variables in our analyses operate as mediators and illegitimately "explain away" some part of the effect. We therefore also ran all analyses without these measures. Moreover, we ran additional analyses that included a country-standardized income measure for the countries that provide this information. The results of both analyses do not differ substantially from those presented below.

To account for self-selection into unionized jobs, we also ran models that included individuals' probability of joining a union. The likelihood of being a union member was estimated based on country-specific logistic regression analyses using the following predictors: gender, age, education, occupational status, public

sector employment, and supervisory position (see, e.g., Brady [2007], who also used these variables as individual-level predictors for unionization), and also subjective measures of the role of unions for workers' well-being, political orientation, and intrinsic work motivations. The results of these analyses are relatively similar to those in the paper's main analyses.

In order to test for possible problems of multicollinearity in our multilevel models, we ran an OLS regression on country-level averages with only macro-level variables as predictors. Based on this analysis, we did not find any indication of a problem when we examined the variance inflation factors. None of the values for any of our dependent variables were higher than 3.78 (usually values above 5 are considered to be an indicator of multicollinearity; see Kutner, Nachtsheim, and Neter [2004], for example).

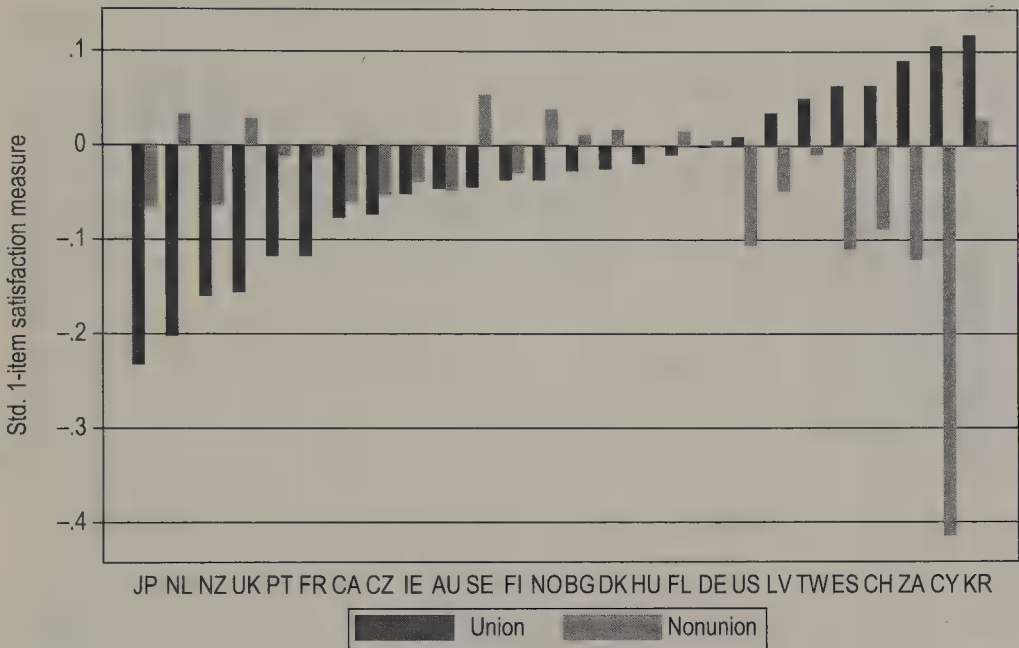
To ensure that the results in our multilevel analyses are not driven by those countries with high numbers of observations, we compared the results based on the maximum likelihood estimator, which we used in the analyses presented in the paper, to those based on the restricted maximum likelihood estimator (REML). The REML estimator tends to provide more conservative estimates for unbalanced data (as is the case with our sample). The results between the two estimation methods do not differ in any substantive way.

Results

Figures 1 to 3 display the standardized country means of workers' job satisfaction by union status. All three figures show that job satisfaction varies considerably across countries (see Sousa-Poza and Sousa-Poza [2000] for similar findings) and that there is no single direction in the relationship between union membership and job satisfaction across countries (see Donegani and McKay [2012] for similar findings). Figure 1, which presents the means of the standardized single-item measure of job satisfaction, indicates that union members in the United States, Latvia, Taiwan, Spain, Cyprus, South Africa, and Switzerland, for example, are on average more satisfied with their jobs than nonmembers, and that their general level of job satisfaction is above the overall country mean. The opposite is true in the Netherlands, the United Kingdom, Sweden, and Norway. Here, union members are on average considerably less satisfied than nonmembers; their general level of satisfaction, moreover, is below the average of all workers in the sample. When we move on to look at satisfaction with particular aspects of the job, that is, satisfaction with job content (figure 2) and material aspects of the job (figure 3), we also see variation across countries.

From figure 2, we can see that union members are, on average, considerably less satisfied with the intrinsic aspects of their jobs (satisfaction with job content) than nonmembers in Japan and Germany, and considerably more satisfied in Bulgaria, Portugal, Latvia, South Africa, and the UK, for example. Likewise, figure 3 shows variation in the relationship between union membership and workers' satisfaction with the material aspects of their work. In Bulgaria and Belgium (Flanders) union members are on average less satisfied with the material aspects of their jobs than nonunion members but more satisfied in the United Kingdom, Canada, Japan, and Cyprus, for example.

Figure 1. Mean job satisfaction by union status and country

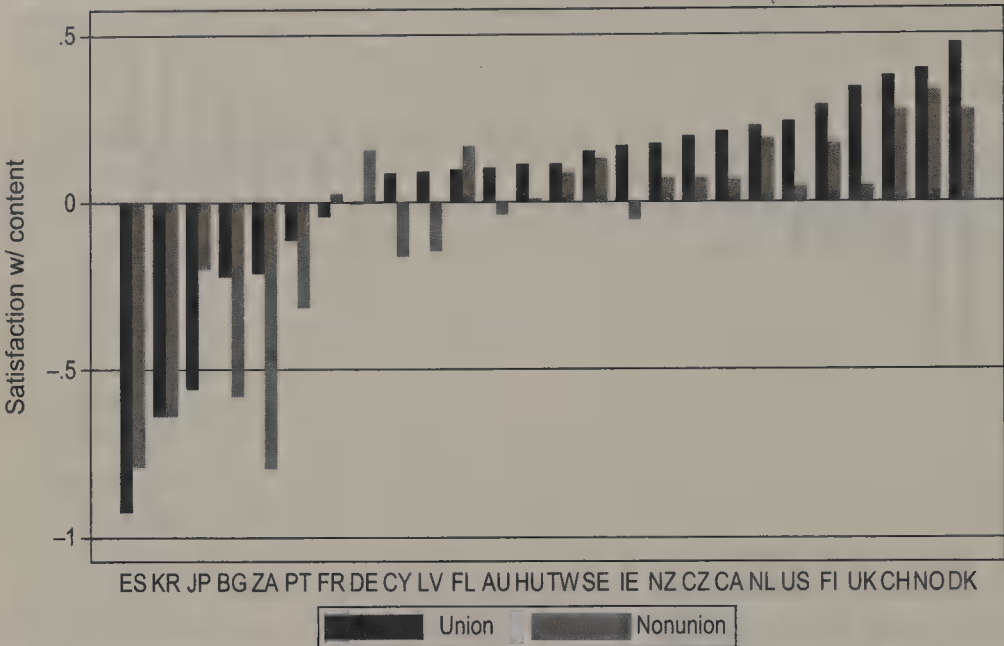


Source: ISSP 2005 ($N = 14,669$). The following country abbreviations are used: AU (Australia), BG (Bulgaria), CA (Canada), CH (Switzerland), CY (Cyprus), CZ (Czech Republic), DE (Germany), DK (Denmark), ES (Spain), FI (Finland), FL (Belgium/Flanders), FR (France), HU (Hungary), IE (Ireland), JP (Japan), KR (South Korea), LV (Latvia), NL (Netherlands), NO (Norway), NZ (New Zealand), PT (Portugal), SE (Sweden), TW (Taiwan), UK (United Kingdom), US (United States), ZA (South Africa).

Simple descriptive statistics thus indicate that union membership does not have the same effect on job satisfaction across different countries. Interestingly, in contrast to previous studies that found a negative association between union membership and job satisfaction for English-speaking countries (e.g., Borjas 1979; Freeman 1978; Kochan and Helfman 1981; Freeman and Medoff 1984), we even find a positive and statistically significant relationship for these countries. However, in the face of the constant decline of unions and union membership in almost all industrialized countries (particularly in English-speaking countries), it may actually not be surprising that unions are positively perceived by those who benefit from improved working conditions and job security. Studies using more recent data to analyze the unionization–job satisfaction relationship support such a “relative deprivation” argumentation (Wunder and Schwarze 2006; Card et al. 2012).

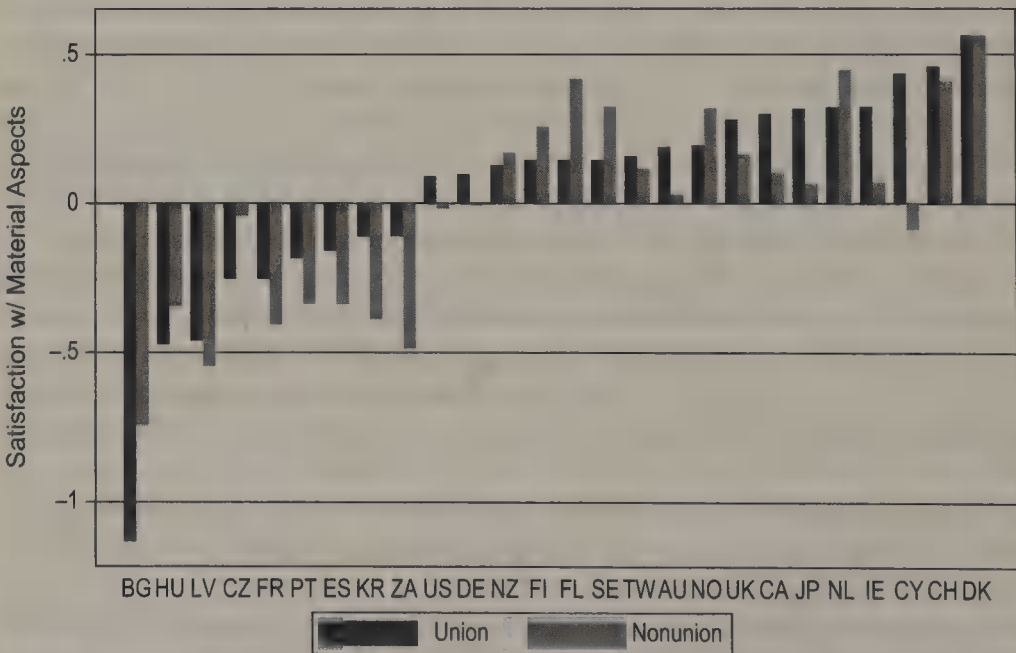
Even after controlling for the relevant individual and job characteristics, as we do in the OLS regressions on each single country (table 3), we still find great variation in the relationship between union membership and job satisfaction: union membership is either positively, negatively, or not at all associated with job satisfaction. The analyses based on the single-item measure of job satisfaction (column 1) indicate a positive association between union membership and job satisfaction for Canada, South Africa, and the United States but no statistically significant association for any of the other study countries.

Figure 2. Mean satisfaction with job content by union status and country



Source: ISSP 2005 (*N* = 14,669). The following country abbreviations are used: AU (Australia), BG (Bulgaria), CA (Canada), CH (Switzerland), CY (Cyprus), CZ (Czech Republic), DE (Germany), DK (Denmark), ES (Spain), FI (Finland), FL (Belgium/Flanders), FR (France), HU (Hungary), IE (Ireland), JP (Japan), KR (South Korea), LV (Latvia), NL (Netherlands), NO (Norway), NZ (New Zealand), PT (Portugal), SE (Sweden), TW (Taiwan), UK (United Kingdom), US (United States), ZA (South Africa).

Figure 3. Mean satisfaction with material aspects of job by union status and country



Source: ISSP 2005 (*N* = 14,669). The following country abbreviations are used: AU (Australia), BG (Bulgaria), CA (Canada), CH (Switzerland), CY (Cyprus), CZ (Czech Republic), DE (Germany), DK (Denmark), ES (Spain), FI (Finland), FL (Belgium/Flanders), FR (France), HU (Hungary), IE (Ireland), JP (Japan), KR (South Korea), LV (Latvia), NL (Netherlands), NO (Norway), NZ (New Zealand), PT (Portugal), SE (Sweden), TW (Taiwan), UK (United Kingdom), US (United States), ZA (South Africa).

Table 3. Estimates of OLS Regressions on the Unionization–Job Satisfaction Relationship by Country

	Job satisfaction (single-item, std.)		Satisfaction with job content		Satisfaction with material aspects of job	
	Est	(SE)	Est	(SE)	Est	(SE)
Canada	0.224**	(0.10)	0.262**	(0.12)	0.609***	(0.10)
United States	0.263***	(0.10)	0.144	(0.09)	0.271***	(0.09)
Ireland	0.048	(0.10)	0.076	(0.10)	0.229**	(0.10)
South Africa	0.204***	(0.07)	0.266**	(0.13)	0.228**	(0.10)
Cyprus	0.051	(0.08)	0.078	(0.11)	0.180*	(0.10)
France	0.001	(0.08)	−0.130	(0.11)	0.180*	(0.10)
South Korea	0.013	(0.11)	−0.190	(0.12)	0.167*	(0.09)
Denmark	0.069	(0.08)	0.218***	(0.06)	0.149*	(0.08)
Australia	0.039	(0.06)	0.015	(0.07)	0.141**	(0.07)
Japan	0.014	(0.11)	−0.304*	(0.16)	0.212	(0.14)
United Kingdom	−0.059	(0.12)	0.230**	(0.10)	0.133	(0.11)
Portugal	−0.065	(0.08)	0.116	(0.11)	0.129	(0.09)
Germany	−0.031	(0.08)	−0.238***	(0.09)	0.122	(0.09)
Latvia	0.081	(0.10)	0.076	(0.11)	0.093	(0.11)
Spain	0.125	(0.12)	−0.313*	(0.18)	0.084	(0.14)
New Zealand	−0.037	(0.09)	0.100	(0.09)	0.072	(0.09)
Taiwan	−0.005	(0.06)	−0.026	(0.06)	0.016	(0.07)
Finland	0.033	(0.09)	0.112	(0.07)	0.012	(0.09)
Norway	−0.040	(0.07)	0.012	(0.06)	−0.033	(0.07)
Sweden	0.028	(0.09)	−0.125	(0.08)	−0.072	(0.08)
Switzerland	0.061	(0.09)	−0.019	(0.07)	−0.096	(0.08)
Czech Republic	0.102	(0.10)	0.123	(0.10)	−0.102	(0.11)
Netherlands	−0.090	(0.10)	−0.010	(0.09)	−0.111	(0.09)
Hungary	−0.118	(0.14)	−0.003	(0.15)	−0.204	(0.15)
Belgium	0.080	(0.08)	0.029	(0.08)	−0.193***	(0.07)
Bulgaria	−0.094	(0.14)	0.119	(0.19)	−0.473***	(0.15)

Note: All individual-level control variables are included in the analyses; standard errors are provided in brackets; individual-level controls explain a considerable proportion of the variance (up to 68 percent for the standardized 1-item measure of job satisfaction, up to 36 percent for satisfaction with the material aspects of the job, and up to 31 percent for the satisfaction with job content); none of the variance inflation factors (VIFs) is bigger than 4.38; multicollinearity does thus not seem to be an issue; due to missing information on relevant country-level characteristics, the number of countries used in this table exceeds the number of countries we use in the multilevel analyses.

Source: ISSP 2005.

* $p < 5.69 < .05$ ** $p < .01$ *** $p < .00$

Likewise, we do not find a lot of evidence for differences in the relationship between job satisfaction and union membership when we examine workers' happiness with the content of their jobs (column 2): union members in Canada, Denmark, South Africa, and the United Kingdom seem to be happier with the organization and the type of work they do, while union members in Germany and Japan are less happy than nonunion members. For the remaining countries of our study, we do not find a statistically significant difference between union members and nonunion members once we include the relevant control variables in our model.

More variation can be observed when we examine the association between union membership and workers' satisfaction with the material aspects of their jobs (column 3). Controlling for relevant demographic and workplace characteristics, unionized workers in Belgium (Flanders) and Bulgaria are on average less satisfied with the material aspects of their jobs, while union members in Australia, Canada, Cyprus, Denmark, France, Ireland, South Korea, South Africa, and the United States are more satisfied than their nonunionized counterparts. For the Czech Republic, Finland, Germany, Hungary, Japan, Latvia, the Netherlands, New Zealand, Norway, Portugal, Spain, Sweden, Switzerland, Taiwan, and the UK, our data indicate no statistical differences in levels of job satisfaction between union and nonunion members when it comes to judging the material aspects of work.⁷

Although previous research has created instructive country groupings of union membership and union strength (see in particular the work of Brady [2007]; Western [1997]), the unionization–job satisfaction relationship does not fit these categories. It is therefore necessary to further examine the question of whether differences in countries' industrial relations systems explain variations in the union–job satisfaction relationship. We therefore now turn to the results of our multilevel analyses (table 4). Based on the empty model of each of our dependent variables, that is, the model that includes only the intercept (not shown), we calculated the intra-class correlation ρ (ICC). The ICC indicates how much of the total variance in the data is due to country-level differences. While the ICC of the model with single-item job satisfaction measure is only 0.03 (which is not surprising, given that a single-item measure usually tends to vary less across countries than composite measures), the ICC on workers' satisfaction with the material aspects of their jobs is 0.12 and their satisfaction with the job content is 0.09, which indicates that country-level differences do indeed play a major role in how workers feel about their jobs.

The first two columns in table 4 present the results of our single-item job satisfaction measure.⁸ We find that higher union density is associated with lower job satisfaction ($p < 0.05$), and that a more cooperative industrial relations climate is associated with higher levels of job satisfaction ($p < 0.01$). The relationships between job satisfaction and both bargaining coverage and bargaining centralization are not statistically significant. Likewise, none of the interaction terms between our macro-level variables and union membership are statistically significant. This is not surprising, given that the OLS regressions/ordered logit regressions on the single-item measure of job satisfaction showed that the relationship between union membership and job satisfaction was also not statistically significant in most countries. In both models, the coefficients of the

individual-level control variables (not shown) and country-level control variables all point in the expected direction.

In the third and fourth columns in table 4, the results on workers' satisfaction with the content of their work are presented. While all individual-level characteristics have the expected and, most often, a statistically significant association with job satisfaction (again, these results are not shown), none of the macro-level indicators and none of the interaction terms between union membership and the macro-level indicators are statistically significant at $p < 0.05$. Again, this null finding is in line with the results of the single-country OLS regressions and consistent with previous studies (e.g., Kochan and Helfman 1981; Meng 1990; Gomez-Mejia and Balkin 1984). Though unions can improve workers' wages and job security, we have little reason to believe that they influence how workers assess the usefulness of their jobs or their job autonomy. Hence, union status should also not influence workers' satisfaction with job content.

In the last three columns of table 4, the results on workers' satisfaction with the material aspects of their work are shown. We can see that the characteristics of the industrial relations system play a role in workers' job satisfaction. A cooperative industrial relations climate is positively associated with workers' satisfaction with the material aspects of their jobs ($p < 0.001$), as is greater bargaining coverage ($p < 0.001$). Turning to the results of the cross-level interaction effects to examine the association between country-level characteristics and the unionization-job satisfaction relationship, we can see that union members seem to be happier with their jobs than nonunion members, the higher the collective bargaining coverage is ($p < 0.001$), and that they are unhappier, the more centralized the collective bargaining system is ($p < 0.001$). Both findings go against our initial expectations.

While we expected higher levels of bargaining coverage to be associated with greater job satisfaction for workers in general (and this is what the analyses in column 5 and the main effect of bargaining coverage in column 6 show), the result that union members are even more satisfied with the material aspects of their jobs than nonunion members with increasing levels of bargaining coverage is somewhat surprising. If being a union member is not associated with a wage premium, why should union members be more satisfied with the material aspects of their jobs than nonunion members? One possible explanation for this finding relates to the fact that higher levels of bargaining coverage tend to be associated with lower wage dispersion (Wallerstein 1999). If workers—particularly union workers, who may be more aware of working conditions overall—have less reason to feel that they are faring worse than others, they may also be happier with their jobs.

The fact that unionized workers seem to be less happy with the material aspects of their jobs with a more centralized collective bargaining system is also somewhat puzzling. One explanation for this finding could be that union members in countries where wages and working conditions are negotiated at a lower level may be more aware of what their union has done for them. In addition, union members may realize that they are better off than their nonunionized colleagues, which should further contribute to their job satisfaction.

To further explore these two rather counterintuitive results and test our explanations, we examined the joint effect of bargaining centralization and bargaining

Table 4. Estimates of Linear Multilevel Regression Analyses of Single-Item and Composite Job Satisfaction Measures with Union Membership as Random Slope

	Satisfaction			Satisfaction with content			Satisfaction with material aspects of work													
	Est	(SE)		Est	(SE)		Est	(SE)		Est	(SE)									
Fixed part																				
Union membership	0.022	(0.02)		0.173	(0.21)		0.01	(0.03)		0.466†	(0.24)		0.029	(0.04)		-0.068	(0.22)		0.235	(0.22)
Union Density	-0.004*	(0.00)		-0.004*	(0.00)		0.003	(0.00)		0.003	(0.00)		-0.001	(0.00)		-0.001	(0.00)		-0.001	(0.00)
Unemployment rate 2005	0.042*	(0.02)		0.042*	(0.02)		-0.02	(0.04)		-0.021	(0.04)		-0.007	(0.02)		-0.007	(0.02)		-0.005	(0.02)
Dismissal protection	-0.006*	(0.00)		-0.005*	(0.00)		0.00	(0.00)		0.00	(0.00)		-0.006*	(0.00)		-0.006*	(0.00)		-0.006**	(0.00)
Industrial relation climate	0.140**	(0.05)		0.143**	(0.05)		0.064	(0.1)		0.074	(0.1)		0.248***	(0.06)		0.243***	(0.06)		0.227***	(0.05)
Bargaining coverage	0.003†	(0.00)		0.003	(0.00)		0.003	(0.00)		0.003	(0.00)		0.008***	(0.00)		0.007***	(0.00)		0.003	(0.00)
Level of CB	0.04	(0.04)		0.051	(0.04)		-0.093	(0.07)		-0.094	(0.07)		-0.047	(0.04)		-0.024	(0.04)		-0.151*	(0.07)
Union × Density				0.000	(0.00)			(0.00)		0.003	(0.00)			(0.00)		-0.003†	(0.00)		-0.004**	(0.00)
Union × IR climate				-0.008	(0.04)			(0.04)		-0.080†	(0.05)			(0.05)		0.058	(0.04)		0.053	(0.04)
Union × Coverage				0.00	(0.00)			(0.00)		-0.002	(0.00)			(0.00)		0.005***	(0.00)		0.001	(0.00)
Union × Level				-0.017	(0.04)			(0.04)		0.042	(0.05)			(0.05)		-0.170***	(0.04)		-0.329***	(0.07)
Union × Dismissal protection				-0.003	(0.00)			(0.00)		-0.004†	(0.00)			(0.00)		-0.001	(0.00)		-0.001	(0.00)
Coverage × Level																			0.002*	(0.00)
Union × Coverage × Level																			0.002*	(0.00)
Random part																				
√Ψ ₁₁	0.090	(0.02)		0.090	(0.02)		0.176	(0.03)		0.177	(0.03)		0.103	(0.02)		0.102	(0.02)		0.080	(0.02)
√Ψ ₂₂ [Union membership]	0.039	(0.04)		0.000	(0.00)		0.102	(0.03)		0.070	(0.03)		0.129	(0.03)		0.053	(0.03)		0.000	(0.00)

$\sqrt{\Theta}$	0.956	(0.01)	0.956	(0.01)	0.869	(0.01)	0.868	(0.01)	0.868	(0.01)	0.868	(0.01)
Model fit												
LR Chi square	4131.14		4141.47		1336.88		1746.47		1783.98		1904.18	
bic	31199.39		31239.72		29096.52		29052.46		29080.81		29086.17	
aic	30972.26		30975.95		28869.39		28825.33		28817.05		28807.76	
lrtest	9.26		6.31		8.29		34.86***		18.28**		13.29**	
R ² adjusted (level 1)	0.266		0.267		0.107		0.127		0.127		0.126	
R ² adjusted (level 2)	0.795		0.795		0.628		0.909		0.912		0.946	

Note: All models are adjusted for relevant demographic and work characteristics, that is, gender, age, relationship status, presence of children in household, education, part-time work, supervisory position, occupation, public sector employment, self-reported work-life balance, relationship with management and colleagues, recent participation in training courses, working conditions (i.e., exhausting, stressful, dangerous, or hard physical work); standard errors are provided in brackets. $\sqrt{\Psi_{11}}$ is the estimate of the random intercept standard deviation, $\sqrt{\Psi_{22}}$ is the estimate of the random slope standard deviation of union membership, and $\sqrt{\Theta}$ is the estimate of the standard deviation of the level-1 residual.

$M1 = 11,234; N2 = 20$

* $p < .1$ ** $p < .05$ *** $p < .01$ *** $p < .001$

coverage and its interaction with union status. As shown in column 7, both the two-way interaction between collective bargaining coverage and bargaining centralization and the three-way interaction term between union status, collective bargaining coverage, and bargaining centralization are positive and statistically significant. This finding provides support for the suspicion that workers' job satisfaction, and in particular union members' job satisfaction, increases if the collective bargaining system contributes to a more equal wage distribution by ensuring wide coverage and centralizing the wage coordination process (Wallerstein 1999; Rowthorn 1992).⁹

Conclusions

This paper reexamined the relationship between union membership and job satisfaction. While the debate about unions has been dominated by the narrative that unions are bad for workers' job satisfaction, the results of numerous empirical studies have been inconclusive. This study further investigated the nature of the union–job satisfaction relationship in a cross-national perspective. Since most research on the relationship between unionization and job satisfaction is based on analyses of single countries or establishments (Garcia-Serrano [2009] and Donegani and McKay [2012] are two of the few exceptions), and is therefore unable to take differences in unionism into account, our comparative approach fills an important gap in the existing literature.

First, in our analyses of a comprehensive data set drawn from 26 industrialized countries, we show that the unionization–job satisfaction relationship varies across countries. In some

countries, union members are more satisfied with their jobs; in others, they are less satisfied than nonunion members. However, in most of the countries under investigation, union status does not have a statistically significant association with job satisfaction at all.

Second, by distinguishing between two dimensions of job satisfaction, that is, individuals' satisfaction with the material aspects of their job and individuals' satisfaction with the content of their job, we move beyond those studies that are based on single-item measures of job satisfaction or scales summarizing these dimensions into one single indicator. Our analyses show that only workers' satisfaction with the material aspects of their jobs varies by union status. Substantively, this means that unions matter for those aspects of job satisfaction that they can more readily change.

Third, by combining individual-level data with country-level information, we were also able to show that the actual *effect unions have on workers' job satisfaction* is contingent on *what they can do*. Country-level differences are an important factor in explaining why some workers are happier than others. In particular, we find that greater bargaining coverage and a more cooperative industrial relations climate seem to have a positive influence on both unionized and nonunionized workers' satisfaction with the material aspects of their work. While union members' satisfaction with the material aspects of their work is negatively associated with bargaining centralization, it is positively associated with bargaining coverage. Hence, unionism seems to have not only a direct but also an indirect effect on workers' job satisfaction. Strong unions can, for example, influence policies, and under certain circumstances they can pressure legislators to pass policies that are favorable to their members and workers more broadly (Pizzorno 1978). Historically, we have seen a good deal of union influence on many welfare state institutions, including on the healthcare and unemployment insurance systems in numerous countries, as well as over areas like wage determination and restraint (Esping-Andersen 1990; Rothstein 1992; Streeck 1992; Swank and Martin 2001). These areas of policy influence may make all workers more satisfied but are unlikely to affect union members differently from nonmembers.

While our study showed that the relationship between union membership and job satisfaction differs across countries and hence is even more complex than was shown in previous studies, our analyses of cross-national data do not allow us to draw causal inferences. Future studies should therefore probe the unionization–job satisfaction relationship longitudinally and more systematically examine interactions between union membership and the national-level characteristics of industrial relations systems. The linkage between union membership and job satisfaction may vary not only across context but also over time, which is another plausible explanation for the inconsistent findings in the existing literature. For instance, changes in unemployment rates (rather than unemployment levels), collective bargaining characteristics (e.g., union density), and laws (e.g., the passage of right-to-work laws) might affect how unionized workers view their position vis-à-vis nonunionized workers and hence also their job satisfaction, as recent work on union-related institutional changes (see Baccaro and Howell 2011) and changes in workers' value systems (see Kalleberg and Marsden 2013) suggests.

In sum, our expanded view of job satisfaction, which takes into account overall satisfaction but also satisfaction with specific aspects of a job, revealed the

aspects of workers' job satisfaction that are amenable to improvement and showed what unions can, and importantly cannot, do to improve employees' satisfaction with their jobs. As workers' happiness and well-being are associated with lower staff turnover and absenteeism, the insights of this study are thus equally relevant for unions, employers, and policymakers.

Notes

1. We also used a multiplicative coding scheme for the construction of our dependent variable to test the robustness of our measure. The results of the analyses using this alternative coding scheme (available upon request), however, did not substantially differ from those that we present here.
2. CESifo compiled from the World Economic Forum Global Competiveness Report, which uses their "executive opinion survey." The survey asks: "How would you characterize labour-employer relations in your country?" (1 = generally confrontational; 7 = generally cooperative) (CESifo Group Munich 2010). This scale is quite robust, as Chor and Freeman (2005) found while conducting a similar survey with a very different set of respondents—scholars rather than businesspeople.
3. Since the coverage data for Bulgaria and Latvia are not available for 2004 and 2005, the mean coverage rates for the years 2003 and 2006 are used. Likewise, for the Czech Republic, the mean of the years 2004 and 2006 is used since data for 2005 are not available. For Portugal, information from 2006 is used, since no data are available for 2005.
4. The "strength of dismissal protection" variable reflects the degree to which employers face legal regulation into how they hire and fire workers, for example whether the default employment relationship is "at will," as in the United States, or whether generally employees can only be fired "for cause," as is the case in many other countries.
5. The data from the Netherlands are not integrated in the original data file but can be downloaded separately from the GESIS website (www.issp.org).
6. We use random slope models rather than a random intercept model because the country-specific regression models show that the relationship between being a union member and job satisfaction (see table 3) varies across countries. Moreover, we compared the model fit of random intercept and random slope models. The likelihood ratio tests for all three dependent variables indicated that those models using union status as a random coefficient fit the data better than those implementing union status as a fixed-effects component.
7. These relationships persist when we include individuals' probability of joining a union as an additional predictor (see note 6).
8. In addition to the linear random coefficient model, we also applied a proportional odds model to take the ordinal structure into account; the results of these analyses, which are available upon request, however, do not substantially differ from the linear models.
9. However, it is important to note here that it is not wage equality per se but unions' contribution to a more equal wage distribution that is positively associated with union members' job satisfaction. When adding the Gini coefficient as an additional control variable in our model, we can see that union members are actually more satisfied with their jobs than nonunion members with increasing levels of wage inequality (while the interaction terms basically retain their direction and level of statistical significance). This finding affirms the results of the single-country analyses, particularly the results of a positive relationship between union membership and job satisfaction for the English-speaking countries, presented in table 3.

Appendix

Appendix A. Questions Underlying Composite Job Satisfaction Measures

Original questions	Answer categories
How satisfied are you in your (main) job?	completely satisfied very satisfied fairly satisfied neither satisfied nor dissatisfied fairly dissatisfied very dissatisfied completely dissatisfied
For each of the following, please tick one box to show how important you personally think it is in a job. How important is ... a ... job security b ... high income c ... good opportunities for advancement d ... an interesting job e ... a job that allows someone to work independently f ... a job that allows someone to help other people g ... a job that is useful to society	very important important neither important nor unimportant not important not important at all
For each of these statements about your (main) job, please tick one box to show how much you agree or disagree that it applies to your job. a. My job is secure. b. My income is high. c. My opportunities for advancement are high. d. My job is interesting. e. I can work independently. f. In my job I can help other people. g. My job is useful to society.	strongly agree agree neither agree nor disagree disagree strongly disagree

Appendix B. Factor Analysis

Factor analysis/correlation			Number of obs = 11,234	
Method: principal-component factors			Retained factors = 2	
Rotation: (unrotated)			Number of params = 13	
Factor	Eigenvalue	Difference	Proportion	Cumulative
Factor 1	2.63727	1.41467	0.3768	0.3768
Factor 2	1.22260	0.40275	0.1747	0.5514
Factor 3	0.81985	0.13201	0.1171	0.6685
Factor 4	0.68783	0.08476	0.0983	0.7668
Factor 5	0.60307	0.06844	0.0862	0.8529
Factor 6	0.53464	0.03989	0.0764	0.9293
Factor 7	0.49475	—	0.0707	1.0000

Note: LR test: independent vs. saturated: $\chi^2(21) = 1.4e + 04$; Prob > $\chi^2 = 0.0000$.

Factor loadings (pattern matrix) and unique variances

Variable Satisfaction with ...	Factor 1	Factor 2	Uniqueness
... income	0.6335	0.4924	0.3562
... advancement opportunities	0.6575	0.3587	0.4390
... job autonomy	0.6107	-0.1146	0.6140
... helpfulness of work	0.6406	-0.5045	0.3350
... usefulness of work	0.5671	-0.5496	0.3763
... interestingness of work	0.6935	-0.1212	0.5044
... job security	0.4667	0.5168	0.5151

Factor analysis/correlation

Method: principal-component factors

Retained factors = 2

Rotation: orthogonal varimax (Kaiser off)

Number of params = 13

Factor	Variance	Difference	Proportion	Cumulative
Factor 1	1.94090	0.02193	0.2773	0.2773
Factor 2	1.91897	-	0.2741	0.5514

Note: LR test: independent vs. saturated: $\chi^2(21) = 1.4e + 04$, Prob > $\chi^2 = 0.0000$ **Rotated factor loadings (pattern matrix) and unique variances**

Variable Satisfaction with ...	Factor 1	Factor 2	Uniqueness
... income	0.1060	0.7953	0.3562
... advancement opportunities	0.2168	0.7169	0.4390
... job autonomy	0.5155	0.3468	0.6140
... helpfulness of work	0.8105	0.0900	0.3350
... usefulness of work	0.7897	0.0063	0.3763
... interestingness of work	0.5792	0.4001	0.5044
... job security	-0.0301	0.6957	0.5151

Factor rotation matrix

	Factor 1	Factor 2
Factor 1	0.7126	0.7016
Factor 2	-0.7016	0.7126

 $N = 11,234$

About the Authors

Lena Hipp is the head of the research group “Work and Care” at the WZB Berlin Social Science Center (Germany). Her research on nonstandard employment, workers’ attitudes and perceptions, and the work–care interface has been published in *Socio-Economic Review*, *International Journal of Social Welfare*, *Social Policy and Administration*, *Advances in Group Processes*, and *Labor Studies Journal*.

Rebecca Kolins Givan is Associate Professor of Labor Studies and Employment Relations in the School of Management and Labor Relations at Rutgers University, where she also codirects the Center for Work and Health. Her comparative research has been published in several edited volumes and journals, including the *British Journal of Industrial Relations*, *ILR Review*, *Labor Studies Journal*, and *International Journal of Human Resource Management*.

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Time Period, Generational, and Age Differences in Tolerance for Controversial Beliefs and Lifestyles in the United States, 1972–2012

Jean M. Twenge, *San Diego State University*

Nathan T. Carter and W. Keith Campbell, *University of Georgia*

Americans have become increasingly tolerant of controversial outgroups in results from the nationally representative General Social Survey (1972–2012, $N = 35,048$). Specifically, adults in the 2010s (versus the 1970s and 1980s) were more likely to agree that Communists, homosexuals, the anti-religious, militarists, and those believing Blacks are genetically inferior should be allowed to give a public speech, teach at a college, or have a book in a local library. Cross-classification hierarchical linear modeling (HLM) analyses separating the effects of time period, cohort/generation, and age show that these trends were driven by both a linear time period effect and a curvilinear cohort effect, with those born in the late 1940s (Boomers) the most tolerant when age and time period were controlled. Tolerance of homosexuals increased the most, and tolerance of racists the least. The increase in tolerance is positively correlated with higher levels of education and individualistic attitudes, including rejecting traditional social rules, but is negatively correlated with changes in empathy.

Suppose someone with controversial views came to your community. Would you favor allowing them to give a speech? Would you want them to teach at a local college or university? Should a book expressing their views be banned from your local library? The General Social Survey (GSS), a nationally representative sample of adult Americans collected since 1972, asked these questions about five different controversial views or lifestyles: homosexuals, Communists, anti-religious atheists, militarists, and racists. These questions get to the heart of recent debates around tolerance, acceptance, individual rights, and freedom within liberal democracies. In this paper, we examine whether tolerance for controversial views and lifestyles has changed over time. That is, are people today more tolerant than they were in the 1970s, and of which groups? For the purposes of this paper, we define tolerance as agreeing that controversial outgroups should be allowed public expression. Tolerance is an important indicator of how societies treat people with views and lifestyles divergent from their average members.

Address correspondence to Jean M. Twenge, Department of Psychology, San Diego State University, 5500 Campanile Drive, San Diego, CA 92182-4611; E-mail: jtwenge@mail.sdsu.edu.

If tolerance has changed, a second, equally important, question is the mechanism behind that change. Populations change over time in three ways: *time period* (a cultural change that affects people of all ages), *birth cohort/generation* (a cultural change primarily affecting young people that is retained with age), and *age* (developmental effects; see Yang [2008]). That is, has tolerance changed because people of all ages and generations change at the same time (a time period effect), because new generations enter the survey and older generations exit (a generational or cohort effect), or because the American population has aged (a developmental effect)? (Note that birth cohort refers to everyone born in a given year, and generation to those born within a specified period. Both refer to the effects of being born during a certain era and are thus somewhat interchangeable; we will use generation most of the time but will refer to birth cohort when we are specifically referring to birth year). Until recently, it was difficult to separate the effects of time period, generation, and age, as each variable is a function of the other two and thus cannot be simultaneously entered into a regression equation. However, hierarchical linear modeling (HLM) techniques now allow the separation of the three effects (e.g., Yang 2008). Teasing apart the influence of these three variables allows for a more precise determination of the origins of social change. If the differences are due to generation, tolerant attitudes formed during childhood are likely the main driver of social change. In contrast, if the differences are due to time period, the entire culture and those of all ages change, not just the young. Finally, developmental effects suggest that any cultural change is due to an overall shift in age of the population, such as a “graying” population or “youth bulge.”

Third, why does tolerance change—which demographic and attitudinal changes co-occur with tolerance? Several possible models could explain an increase in tolerance, including increasing education and contact across groups (Kozloski 2010; Ohlander, Batalova, and Treas 2005; Williams, Nunn, and Peter 1976), differential mating strategies linked to greater independence after formal education (Rosenfeld 2007), genetic changes from migration or differential birth/mortality rates or environmental changes (Rentfrow 2010), drug use (Compton et al. 2006), or even decreasing parasite load (Murray, Trudeau, and Schaller 2011). Finally, rising individualism may be linked to increasing tolerance. We discuss this possibility in more detail below, as it was the impetus for this research. However, we acknowledge that it is very difficult to answer the “why” question adequately—the data over time are limited, those that exist are correlational, and multiple intertwined causes are likely at work at the same time.

To summarize, we have three primary goals. First, we seek to determine whether tolerance changed over time. Second, we explore the nature of this effect—is it primarily a time period effect, a generational effect, or an age effect? Third and finally, we examine the correlates of tolerance to determine if they are consistent with a rise in individualism and/or other factors.

Placing Tolerance in the Broader Context of Individualism

Changes in attitudes and values over time periods and generations are rooted in cultural change (Elder 1974; Twenge, Campbell, and Gentile 2012a). Cultures

and individuals mutually influence and constitute each other (Markus and Kitayama 2010). Much of the work on cross-cultural differences has focused on individualism (a cultural system that favors the needs or desires of the individual over those of the group) versus collectivism (which favors the group over the individual; e.g., Markus and Kitayama [1991]). This model can also be applied within cultures to explain cultural shifts over time and generations (Twenge 2014). For example, several authors have argued that Western cultures have become more focused on the individual self over time, beginning in the Renaissance (Baumeister 1987) and accelerating in the second half of the 20th century (Fukuyama 1999; Myers 2000). Growing individualism appears, for example, in the more individualistic and less collectivistic language used in American books (Greenfield 2013; Oishi et al. 2013; Twenge, Campbell, and Gentile 2012b, 2013). Among individual people, outcomes have included more positive self-views (Twenge, Campbell, and Gentile 2012a) and lower collectivistic traits, such as empathy (Konrath, O'Brien, and Hsing 2011) and concern for others (Twenge, Campbell, and Freeman 2012), among recent generations (e.g., the Millennials, born after 1982) compared to their predecessors (Boomers, born 1946–1964, and GenX, born 1965–1981). Parents from more recent generations are less likely to value collectivistic traits such as obedience, responsibility, and religious faith in children, and more likely to value individualistic traits such as independence and imagination (Park, Coello, and Lau 2014; Trifan, Stattin, and Tilton-Weaver 2014).

Here, we explore whether the growth in individualism has extended to tolerance for outgroups. Cross-culturally, more individualistic countries are more tolerant of difference and more accepting of broader roles for individuals regardless of race or gender (e.g., Brandt 2011; Chia et al. 1994; Gibbons, Stiles, and Shkodriani 1991; Hadler 2012; West and Hewstone 2012). However, it is unclear whether this tolerance extends to the more marginalized outgroups examined in the GSS survey (e.g., the anti-religious, Communists, racists). Certainly, the changes of the past few decades—such as increasing public support for the legalization of gay marriage—suggest that tolerance has risen. However, the GSS items focus on tolerance unconnected to marriage rights, including accepting outgroup members as public speakers, authors, and teachers who may influence young people, and includes groups other than gays and lesbians.

On the other hand, more individualism and less collectivism may not automatically mean more tolerance. Perhaps less empathy and less concern for others could mean less tolerance for others. More self-focus and individualism could lead people to more readily reject views with which they disagree. That has led some to propose the converse of this idea, reasoning that because Millennials are more tolerant of others, the findings showing they are lower in empathy and concern for others must be wrong (e.g., Arnett 2013). In addition, those who embrace individualistic values may be more likely to have negative attitudes toward minority groups, perhaps because they believe in individual self-sufficiency and thus oppose programs such as affirmative action (Katz and Hass 1988). In other words, if individualism promotes the idea of “every man for himself,” outgroups may be perceived negatively if they are seen as asking for “special treatment.”

A contrasting hypothesis that we endorse posits that individualism should lead to more tolerance even if concern for others has declined. First, individualistic systems value self-expression, so members should generally be tolerant of others' expression and choices (e.g., Kim and Sherman 2007). For example, individualistic systems place fewer restrictions on sexual behavior. Second, when people are treated as individuals—rather than members of distinct groups—tolerance based on group membership should be higher. For example, individualistic cultural systems usually reject social rules restricting the actions and opportunities of groups such as racial minorities and women (e.g., Brandt 2011). Third, individualistic systems allow more contact with outgroup members, which should increase tolerance (e.g., Binder et al. 2009; Madon et al. 2001).

This individualism hypothesis also suggests a model for correlates of tolerance. If tolerance is related to individualism, it should correlate positively with indicators of individualism such as the rejection of traditional social rules (e.g., support of organized religion, taboos against drug use, and restrictions on premarital sex). That also suggests tolerance will be linked to less empathy, not more, as lower empathy is linked to individualistic personality traits (Watson, Biderman, and Sawrie 1994). Empathy (usually defined as understanding the feelings of another) is not the same as tolerance (granting rights to others, which does not necessarily include understanding). Tolerance should also correlate with years of education, as education—especially at the college level—promotes greater individualism (Ohlander et al. 2005).

Economic factors may also be important. If economic prosperity leads to greater individualism and postmodernism (e.g., Greenfield 2009; Inglehart and Welzel 2005), the annual unemployment rate should be negatively correlated with tolerance. Tolerance may also be linked to income inequality, though the direction of the effect is not clear a priori. A society with more income inequality may be a more individualistic, and thus more tolerant, one. Conversely, income inequality could promote less tolerance, with more competition among groups for scarce resources.

Previous Research

Several studies have documented increases in support for equal rights for racial minorities and women (e.g., Carter 2010; Koenig et al. 2011; Twenge 1997). However, it is not clear if this increased tolerance extends to the outgroups examined in the current paper, such as Communists or atheists, who remain very small minorities and thus may be seen as more marginalized and controversial (more “out” as an outgroup). According to recent polling data, atheists are only about 2 percent of the population in the United States, with an additional 3 percent identifying as agnostics. The number of self-identified Communists in the United States is likely to be just as small or smaller (e.g., Lipset and Marks 2001). With questions about five different outgroups, these questions provide a broad view of tolerance of diverse opinions and lifestyles.

Previous research on this topic is intriguing but incomplete. Persell, Green, and Gurevich (2001) found that tolerance for homosexuals increased in the GSS between 1972 and 1994, but it is unclear how responses to these questions have

changed in the past 18 years. They also did not examine whether the change was due to time period, generation, or age. Danigelis, Hardy, and Cutler (2007) examined the GSS items on tolerance through 2004, finding considerable changes in tolerance even among those in their 60s and older. However, their focus was on intra-cohort aging; they specifically note that they did not separate time period, generation, and age effects. Schafer and Shaw (2009) reported increases on a wide range of tolerance markers from 1990 to 2006, including some items from the GSS, though their report was descriptive and did not include effect sizes or secondary statistical analyses. Hadler (2012) examined tolerance in the World Values Survey and found few differences between 1989 and 2010.

Furthermore, the idea that attitudes change over time periods and generations remains an area of debate. Some have questioned the idea that generational or time period differences exist at all, arguing that any perceived changes are due to non-representative sampling or the biased perceptions of older generations (Arnett 2010; Trzesniewski and Donnellan 2010). Where differences do exist, these authors contend, they are usually too small to matter. Therefore, they argue, the idea that generations or time periods are significantly different in their attitudes, values, or personality traits is a myth (Trzesniewski and Donnellan 2010). Thus, it is important to determine whether attitudes change over time and the size of any effects.

The Current Research

Our focus in the present research is on changes in tolerance in the United States, including whether trends are due to time period, cohort/generation, or age. The GSS is a useful sample in which to examine these questions. In addition to being nationally representative, the GSS includes respondents of many ages, allowing the separation of time period, generation, and age effects. We accomplish this through the use of HLM techniques.

Given the increase in cultural individualism in the United States, we predict increased tolerance for controversial groups (Hypothesis 1). Further, if cultural individualism is a key driver of increasing tolerance, we would expect this change to be fairly linear and occur at the cultural level (a time period effect; Hypothesis 2a) and as a linear generational effect (Hypothesis 2b). Finally, we expect that education, religion, political beliefs, income inequality, empathy, cultural individualism, and the rejection of social rules will correlate with tolerance, both at an individual level and when matched by year (Hypothesis 3).

Method

Sample

The GSS is a nationally representative sample of Americans over 18, collected in most years between 1972 and 2012 ($N = 56,859$; for the questions in the current survey, N ranges from 29,631 to 35,048). The GSS data and codebooks are available online (Smith et al. 2013). As suggested by the GSS administrators, we weight the analyses by the weight variable WTSSALL to make the sample nationally

representative of individuals rather than households and correct for other sampling biases. Also as suggested by the administrators, we excluded the Black oversamples collected in 1982 and 1987.

Measures of Tolerance

A section of the GSS asks 15 questions about tolerance for people with controversial views. It begins: "There are always some people whose ideas are considered bad or dangerous by other people. For instance, somebody who is against all churches and religion ... If such a person wanted to make a speech in your (city/town/community) against churches and religion, should he be allowed to speak, or not?" with the two response choices "Yes, allowed" and "Not allowed." The next question was "Should such a person be allowed to teach in a college or university, or not?" with the same two response choices, followed by "If some people in your community suggested that a book he wrote against churches and religion should be taken out of your public library, would you favor removing this book, or not?" with the response choices "Favor" and "Not favor" (with "favor" meaning favoring removing the book). The next questions use the same format to ask about other groups, including "consider a person who believes that Blacks are genetically inferior," "a man who admits he is a Communist," "a person who advocates doing away with elections and letting the military run the country" (a militarist), and "a man who admits that he is homosexual." The question about a Communist teaching in a college is worded slightly differently, asking instead about whether he should be fired. The questions about Communists and the anti-religious were asked since 1972, homosexuals since 1973, and the questions about those believing Blacks were inferior and those advocating military rule were asked since 1976.

Measures of Individualism, Rejection of Social Rules, and Other Characteristics

The GSS does not include a direct measure of individualism. However, in one year (1984) it included an item tapping individualistic and anti-collectivistic beliefs: "In our society everyone must look out for himself. It is of little use to unite with others and fight for one's goals in politics or in unions."

The GSS more consistently included other items that indicate rejection of traditional social rules restricting individual freedom. These included believing marijuana should be legalized, rejecting traditional roles for women (a composite of three items: "A working mother can establish just as warm and secure a relationship with her children as a mother who does not work," "A preschool child is likely to suffer if his or her mother works," and "It is much better for everyone involved if the man is the achiever outside the home and the woman takes care of the home and family"), likelihood of voting for a Black president, choosing "None" for religious affiliation, and expressing approval of premarital sex. Based on past research, we also examined correlations between tolerance and years of education, political party (e.g., Glenn and Weaver 1979), and self-rating as liberal versus conservative.

We also gathered annual statistics on economic factors, cultural individualism, and empathy from publicly available sources and previous research. For economic indicators, we examined the unemployment rate and the GINI index of income inequality. Individualistic words and phrases are from the Google Books database, American books corpus (Twenge, Campbell, and Gentile 2012b). Uncommon names, an indicator of need for uniqueness (a facet of individualism), are from the Social Security Administration database of names; we used boys' names, as they change more linearly over time (Twenge, Abebe, and Campbell 2010). We obtained mean scores by year on empathy among college students from Konrath, O'Brien, and Hsing (2011). We matched these social indicators with the tolerance coefficients by year.

Data Analysis Plan

As a first step, we report descriptive statistics, inferential statistics, effect sizes, and cross-time comparisons by age and birth cohort groups in a table. Data collected over time can be analyzed in many ways, including grouping by 20-year generation blocks, by decades, or by individual year. We felt that separating the data into five-year intervals provided the best compromise between specificity and breadth. We report the effect sizes (d , or difference in terms of standard deviations) comparing the first group of years to the last, but also (1) provide the means and SDs for the five-year intervals between these endpoints, so fluctuations at other times are apparent; and (2) provide a figure with the year-by-year results. We also examine whether the trends are moderated by gender, race, and education level.

In describing the trends in the text, we will occasionally employ labels for the generations such as the GI or "Greatest" generation (born 1900–1924), Silent (1925–1945), Boomers (1946–1964; some argue 1943–1960), GenX (1965–1981 or 1961–1981), and Millennials (1982–1999 or 1977–1999; for reviews, see Strauss and Howe [1991]; Twenge [2014]). These generational birth year cutoffs are arbitrary and are not necessarily justified by empirical evidence, but are common labels for those born in certain eras.

To separate the effects of time period, generation, and age, we perform HLM analyses. Specifically, we use a cross-classified HLM successfully applied to period-cohort-age data in the past (e.g., Yang 2008). This approach involves the estimation of an overall regression line of IRT-estimated tolerance onto the age of respondents; the intercept and slope coefficients of these regression lines are "cross-classified" by birth cohort and survey year in the sense that differences in these coefficients between birth cohorts and survey years are estimated separately. Significant differences in intercepts indicate differences in overall tolerance by birth cohort and/or survey year, whereas differences in slopes indicate that the trajectory of tolerance over the life span differs by birth cohort and/or survey year (for further details, please see the appendix).

Latent Variable Analyses

We also employed several statistical techniques to explore whether the tolerance items could be combined, including factor analysis and item response theory

(IRT). Principal axis factor analysis scree plots suggested that only one factor was necessary. A varimax-rotated, two-factor model showed a less comprehensible solution, and factor plots suggested that all items clustered together on one factor. Therefore, the IRT assumption of unidimensionality (i.e., only one latent variable underlies the survey scores) was upheld.

The two-parameter logistic (2PL) IRT model was fitted to the data using the IRTPRO software program. This model takes into account the extremity/popularity of items (i.e., b , referred to in ability testing as the “difficulty” parameter) and the discriminating power of the item (i.e., the a parameter, which is analogous to an item-total correlation). The 2PL provides a “purified” estimate of the underlying variable that does not include the item-specific idiosyncrasies that would be included in a simple sum-score. The model showed good fit to the tolerance items. Of the 15 tolerance items, none were above the χ^2/df ratio cutoff of 3, ranging from $< .001$ to $.38$, with a mean of $.09$, $SD = .11$. These results suggest that item and person parameters are interpretable. In addition, the Cronbach’s alpha for the 15 items was $.92$. Thus, for the HLM analyses, we relied on the IRT estimate for the composite of the 15 tolerance items.

Results

Change over Time in Mean Tolerance (Hypothesis 1)

In support of Hypothesis 1, tolerance for those espousing controversial views was markedly higher among American adults in the early 2000s and 2010s compared to the 1970s and 1980s, showing a fairly linear increase over time (see table 1 and figure 1). Increases in tolerance for homosexuality were particularly large, with the percentage of those saying a homosexual man should be allowed to teach at a college increasing from 52 percent in 1972–1974 to 85 percent in 2010–2012 ($d = .75$). However, tolerance for racists did not increase very much ($d = .05$ averaged across the three items).

Women ($d = .56$, from 54 percent in 1975–1979 to 71 percent in 2010–2012) increased in total tolerance slightly more than men ($d = .41$, 57 to 70 percent). Whites ($d = .54$, 56 to 74 percent) increased slightly more than Blacks ($d = .42$, 48 to 62 percent). Respondents who did not attend college increased more in tolerance ($d = .41$, from 51 to 64 percent) than those who did ($d = .19$, from 78 to 83 percent). Thus, the largest changes appeared among White men who did not attend college. In both eras, those who attended college were more tolerant.

Time Period, Generation, and Age Differences (Hypothesis 2)

Next, we examined whether the trends were due to time period (survey year), generation (birth year/cohort), or age. Consistent with Hypothesis 2a, the cross-classified HLM analyses revealed that time period (i.e., survey year) was the main driver of variability in total tolerance (see tables 2 and 3 and figure 2a). Thus, Americans of all ages and generations increased in tolerance for out-groups between the 1970s and the 2010s. Consistent with Hypothesis 2b,

Table 1. Changes in Tolerance for Controversial Beliefs and lifestyles in the United States, 1972–2012

	<i>n</i>	72–74	75–79	80–84	85–89	90–94	95–99	00–04	05–09	10–12	<i>d</i>
Anti-religionist											
Allow to speak	35,048	66% (.47)	65% (.48)	67% (.47)	70% (.46)	74% (.44)	75% (.43)	77% (.42)	78% (.42)	77% (.42)	.24*
Allow to teach	34,215	43% (.49)	42% (.49)	47% (.50)	49% (.50)	55% (.50)	60% (.49)	61% (.49)	62% (.48)	63% (.48)	.41*
Allow book in library	34,540	63% (.48)	61% (.49)	64% (.48)	67% (.47)	71% (.46)	72% (.45)	73% (.45)	73% (.44)	76% (.43)	.28*
Homosexuals											
Allow to speak	32,947	65% (.48)	65% (.48)	69% (.47)	72% (.45)	80% (.40)	83% (.37)	84% (.37)	83% (.37)	87% (.33)	.52*
Allow to teach	32,706	52% (.50)	53% (.50)	58% (.49)	61% (.49)	70% (.46)	77% (.42)	80% (.40)	80% (.40)	85% (.36)	.75*
Allow book in library	32,905	57% (.50)	58% (.50)	60% (.49)	61% (.49)	70% (.46)	72% (.45)	74% (.44)	76% (.43)	79% (.41)	.48*
Communists											
Allow to speak	34,611	59% (.49)	57% (.50)	58% (.49)	62% (.49)	69% (.47)	67% (.47)	69% (.46)	68% (.47)	66% (.47)	.15*
Allow to teach	33,468	41% (.49)	42% (.49)	46% (.50)	50% (.50)	58% (.49)	61% (.49)	63% (.48)	62% (.48)	64% (.48)	.47*
Allow book in library	34,292	59% (.49)	58% (.49)	60% (.49)	62% (.49)	69% (.46)	69% (.46)	70% (.46)	70% (.46)	72% (.45)	.27*
Militarist											
Allow to speak	30,251	–	54% (.50)	57% (.50)	58% (.49)	65% (.48)	66% (.47)	67% (.47)	67% (.47)	69% (.46)	.31*
Allow to teach	29,707	–	37% (.48)	41% (.49)	42% (.49)	48% (.50)	52% (.50)	53% (.50)	54% (.50)	58% (.49)	.43*
Allow book in library	29,960	–	57% (.50)	59% (.49)	60% (.49)	67% (.47)	68% (.47)	69% (.46)	71% (.46)	73% (.45)	.33*
Racist											
Allow to speak	30,252	–	61% (.49)	61% (.49)	61% (.49)	64% (.48)	63% (.48)	62% (.48)	62% (.49)	58% (.49)	–.06
Allow to teach	29,816	–	42% (.49)	43% (.50)	44% (.50)	45% (.50)	48% (.50)	49% (.50)	48% (.50)	48% (.50)	.12*
Allow book in library	29,970	–	63% (.48)	64% (.48)	64% (.48)	68% (.47)	66% (.47)	66% (.47)	65% (.47)	65% (.47)	.04
Tolerance composite	25,439	–	56% (.34)	59% (.34)	60% (.33)	66% (.31)	68% (.31)	69% (.30)	69% (.30)	71% (.29)	.47*
Age groups (time-lag design = birth cohort + time period)											
18–29	6,226	–	70% (.30)	66% (.31)	67% (.30)	71% (.29)	73% (.26)	73% (.26)	71% (.26)	72% (.24)	.07
30–39	5,470	–	64% (.33)	72% (.31)	69% (.30)	72% (.29)	71% (.29)	71% (.28)	72% (.27)	75% (.26)	.37*
40–49	4,997	–	53% (.33)	59% (.35)	66% (.32)	72% (.30)	71% (.30)	72% (.29)	72% (.29)	75% (.28)	.72*

(Continued)

Table 1. *continued*

	<i>n</i>	72-74	75-79	80-84	85-89	90-94	95-99	00-04	05-09	10-12	<i>d</i>
50-59	3,900	–	48% (.33)	51% (.34)	53% (.33)	63% (.32)	67% (.31)	71% (.29)	72% (.30)	71% (.30)	.73*
60-69	2,813	–	38% (.32)	43% (.33)	46% (.33)	51% (.32)	59% (.33)	63% (.32)	64% (.32)	68% (.30)	.97*
Over 70	2,250	–	30% (.29)	34% (.30)	38% (.30)	47% (.31)	49% (.34)	52% (.33)	53% (.32)	54% (.31)	.80*
Birth cohort groups (quasi-longitudinal design = age + time period differences)											
Born 1919 or before	2,000	–	37% (.31)	38% (.32)	36% (.30)	44% (.30)	45% (.32)	–	–	–	.26*
Born 1920s	2,330	–	49% (.33)	49% (.33)	48% (.33)	50% (.31)	50% (.34)	50% (.34)	52% (.33)	–	.09
Born 1930s	2,854	–	56% (.33)	58% (.34)	55% (.33)	60% (.32)	61% (.33)	61% (.33)	56% (.33)	55% (.29)	–.03
Born 1940s	4,364	–	66% (.32)	68% (.32)	68% (.32)	70% (.30)	69% (.31)	71% (.30)	67% (.32)	68% (.31)	.06
Born 1950s	5,777	–	69% (.30)	70% (.30)	70% (.30)	74% (.29)	70% (.30)	73% (.28)	73% (.30)	71% (.29)	.07
Born 1960s	4,460	–	–	63% (.31)	67% (.29)	70% (.29)	73% (.28)	70% (.28)	70% (.28)	73% (.29)	.33*
Born 1970s	2,557	–	–	–	–	71% (.27)	73% (.26)	73% (.28)	71% (.27)	75% (.26)	.15*
Born 1980s–1990s	1,254	–	–	–	–	–	–	72% (.25)	70% (.26)	73% (.25)	.04

Note: Cells with dashes indicate either that the question was not asked or that the group had less than 100 respondents during that time period. *d* = difference in standard deviations. At most levels, *d* = 2*r*. * = *p* < .05 or less, *t*-test comparing earliest years to latest years available.

there was also a significant generational effect. However, somewhat inconsistent with the hypothesis, this effect was curvilinear rather than linear, with the first wave of Boomers born in the 1940s expressing the most tolerance, especially compared to the Silent generation born 1925–1945 (see figure 2b). More recent generations, such as Generation X and Millennials, were about average in tolerance when time period and age effects were removed. This can also be seen in the means in table 1; among those 18–29 years old, tolerance did not differ significantly between the mid-1970s (Boomers) and the 2010s (Millennials).

When time period and generation are controlled, tolerance declines with age, with younger respondents more tolerant than older respondents (see figure 2c). Time period also had a significant effect on the linear slopes, β_{1jk} , and quadratic slope, β_{2jk} , of the regression of tolerance onto age. As figure 3 shows, the age trajectory of the tolerance decline is less steep in more recent years. In the 1970s, 18-year-olds were considerably more tolerant than 60-year-olds. In the 2010s, however, these age groups differed little in their level of tolerance (see figure 3). Thus, the time period increase in tolerance

Figure 1. Americans' tolerance (by percentage) for marginalized outgroups, 1972–2012

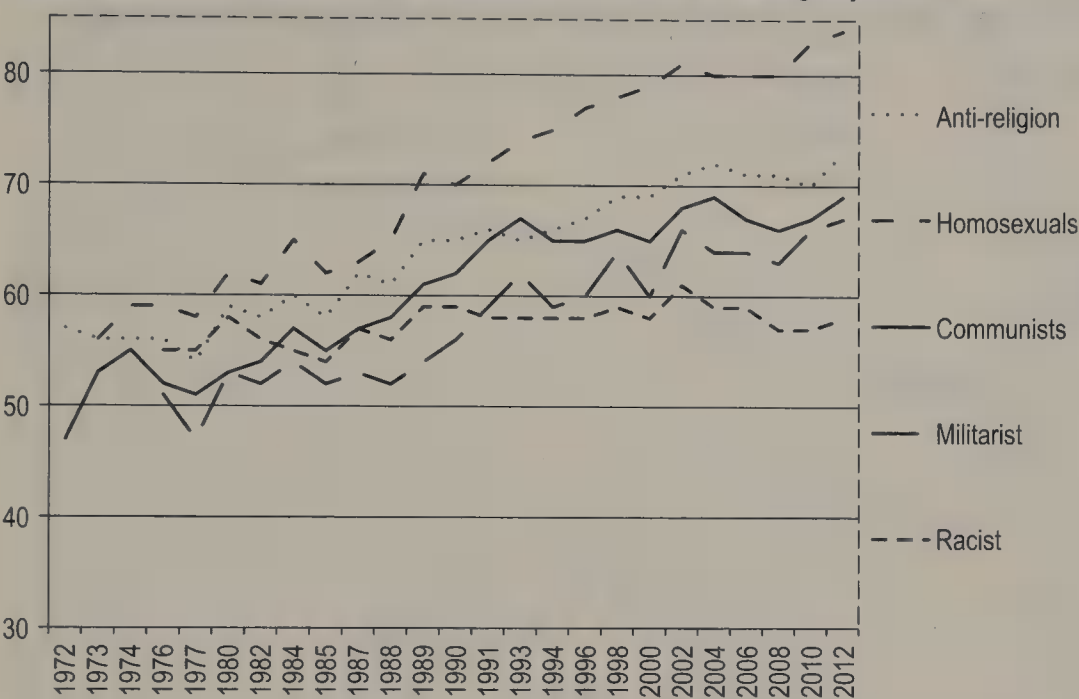


Table 2. Regression Coefficients of Level-1 Age Predictors and Their 95% Confidence Intervals

Fixed effect	Coefficient	95% CIs	
		Low	High
β_{0jk}	.073	−.001	.151
β_{1jk} , Age	−.277	−.333	−.222
β_{2jk} , Age ²	−.104	−.124	−.086
β_{3jk} , Age ³	.035	.021	.050

Table 3. Variance Components for the Effect of Survey Year and Birth Cohort on Level-1 Regression Coefficients

Random effect	σ^2	χ^2	df	p
Variance components for:				
Birth cohort effects	$t_{00k} (\beta_{0jk})$.003	215.84	.007
	$t_{10k} (\beta_{1jk})$	<.001	114.12	.113
	$t_{20k} (\beta_{2jk})$	<.001	108.05	.208
Time period effects	$c_{00k} (\beta_{0jk})$.035	449.84	<.001
	$c_{10k} (\beta_{1jk})$.016	181.43	<.001
	$c_{20k} (\beta_{2jk})$.001	55.77	<.001
	$c_{30k} (\beta_{3jk})$	<.001	68.72	<.001

Figure 2. Americans' tolerance by: (a) survey year (time period), (b) birth year cohort (generation), and (c) age; predicted values estimated by the cross-classified HLM model

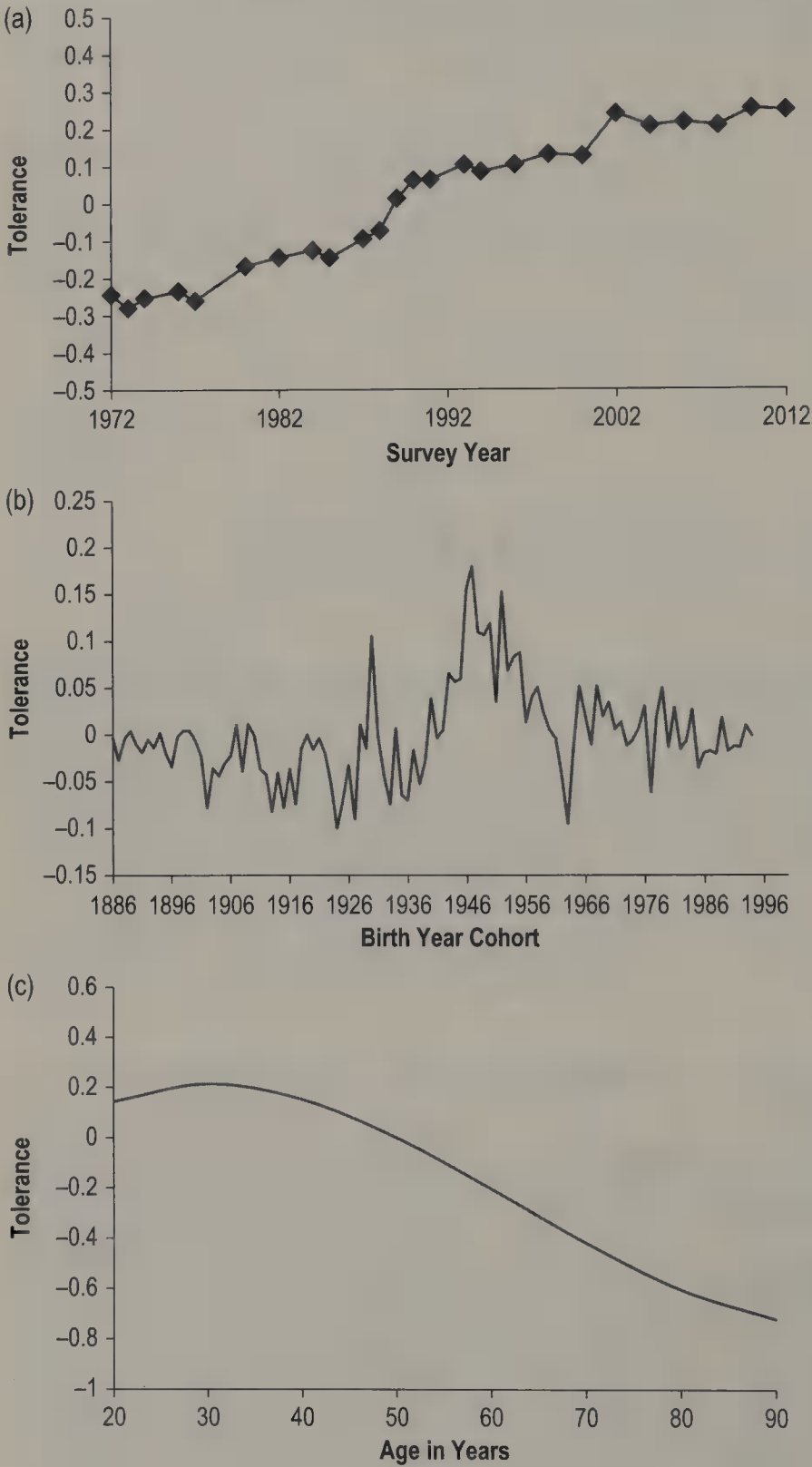
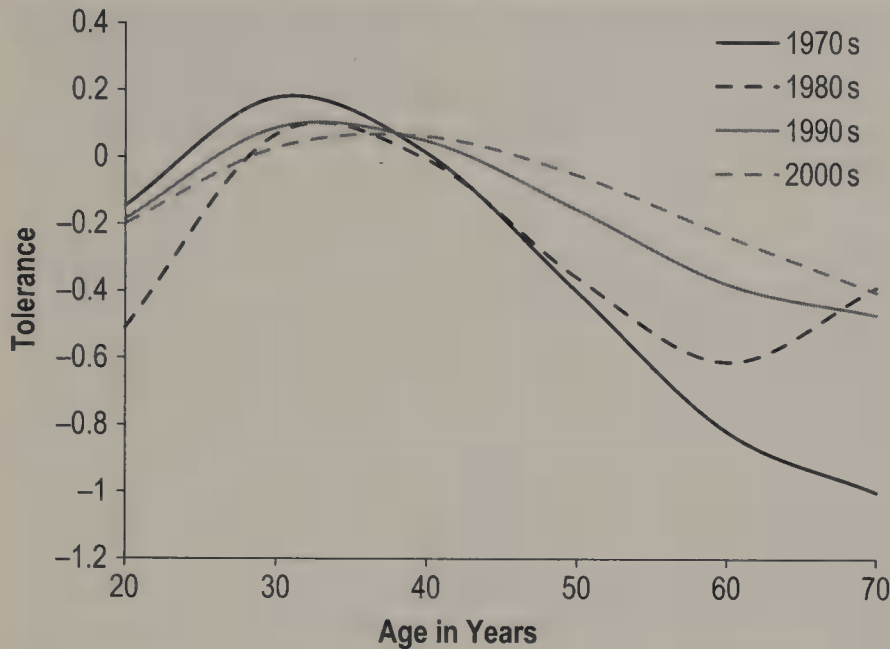


Figure 3. Age trajectories in tolerance by decade of survey

shown in figure 1a is primarily caused by tolerance levels not declining over the lifespan as sharply as they once did.

Predictors of Tolerance (Hypothesis 3)

Consistent with Hypothesis 3, tolerant respondents were more likely to be individualistic, including believing people need to look out for themselves, not affiliating with a religion, believing marijuana should be legal, voting for a Black president, approving of premarital sex, and supporting working mothers (see table 4). Education was the strongest predictor of tolerance. These correlations show that, at an individual level, those who embrace individualistic, egalitarian, and nontraditional views, and who are more highly educated, are also more tolerant.

A second question is whether trends in these attitudes might help explain the increase in tolerance over time. Educational attainment and rejection of social rules also increased over time (see table 4), suggesting that they co-occurred with the increase in tolerance and may be among its possible causes. As a second step, we examined the correlations between mean tolerance (using the coefficients for each year from the HLM analyses) and the means on the other variables matched by year (see, e.g., Twenge, Abebe, and Campbell 2010), as well as other indicators of individualism and economic conditions available at the group level (individualistic language, need for uniqueness, empathy, unemployment, and income inequality). These analyses provide a view of how closely the change in each variable followed the change in tolerance over time at the group level. (These are ecological correlations, but are appropriate, as we are analyzing at the group level for cultural change). These analyses showed that education and all of the variables measuring rejection of social rules and individualism were significant

Table 4. Predictors of Time Period Changes in Tolerance

	With total tolerance	With year	With total tolerance, matched by year
GSS items			
Everyone should look out for themselves (1984)	.20	–	–
No religious affiliation	.19	.13	.87
Favors legalization of marijuana	.28	.14	.70
Would vote for a Black president	.23	.14	.90
Supports working mothers	.31	.15	.80
Premarital sex not wrong	.32	.11	.62
Years of school completed (education)	.42	.21	.96
Political party affiliation (higher = Republican)	.04	.06	.75
Liberal vs. conservative (higher = conservative)	–.14	.02	.56
Social indicators			
GINI index of income inequality	–	.98	.97
Unemployment rate	–	–.10	–.20
Empathy (college students), 1979–2009	–	–.52	–.36
Need for uniqueness (uncommon names)	–	.99	.98
Individualistic words	–	.71	.63
Individualistic phrases	–	.95	.97

Note: Total tolerance is the mean tolerance composite estimate by year, adjusted for cohort and age in the HLM analyses. $r = .02$ significant at $p < .01$; all $r > .03$ significant at $p < .001$.

predictors of tolerance over time (see table 4). Thus, as adult Americans rejected traditional social rules and embraced individualism, tolerance also increased.

Years with more tolerance were also years with less empathy among college students, suggesting that the two constructs—at least at the cultural level—are distinct. Liberal political views were correlated with more tolerance. However, tolerance was only weakly linked to political party, and neither variable was strongly correlated with year. Thus, it is unlikely that changes in political views per se caused the increase in tolerance. Unemployment was weakly but significantly linked with tolerance; years with less unemployment were years with higher tolerance. Income inequality, which has risen steadily over the years, was highly correlated with tolerance. Thus, years with more income inequality were years with higher tolerance.

We also examined the relationship between income inequality and the generational differences in tolerance by matching the birth cohort coefficients with

income equality 20 years later (when respondents were young adults; with the GINI index available beginning in 1967, this included only those born in 1947 and later). The correlation between a birth cohort's tolerance and income inequality was $r(38) = -.52$, $p < .01$, suggesting that tolerance was high among birth cohorts (such as the Boomers) who experienced less income inequality when they were young.

Discussion

Americans have become increasingly tolerant of controversial beliefs and lifestyles (i.e., marginalized outgroups). They are more likely to believe that homosexuals, Communists, militarists, and the anti-religious have the right to give speeches, teach at a college, and have a book in a local library. Smaller increases appeared in tolerance for a person who claims that Blacks are genetically inferior (commonly labeled a racist). Increases in tolerance were larger among respondents who did not attend college. HLM analyses separating the effects of time period, generation, and age (based on Yang [2008]) showed that the increase in tolerance was caused by a combination of time period and generational effects, suggesting that the increase in tolerance is a broad cultural trend. At the generational level, Boomers were more tolerant than other generations when time period and age effects are controlled.

The increase in tolerance co-occurred with increases in individualistic beliefs such as rejecting traditional social rules around gender, race, religion, sexuality, and drug use. At the group level, tolerance was higher in years with more individualistic language in books and a higher need for uniqueness. These analyses cannot infer causation, but these results are consistent with our hypothesis that increasingly individualistic attitudes may be one cause of increasing tolerance for outgroups. Tolerance was also strongly correlated with educational attainment, which has also increased over time. This suggests that as Americans have completed more years of formal education, more have learned tolerance for outgroups.

However, years with higher tolerance were also years with lower empathy among college students. This is not as contradictory as it may seem: tolerance and low empathy are both linked to high individualism (Brandt 2011; Konrath, O'Brien, and Hsing 2011; Watson, Biderman, and Sawrie 1994), which may be the third variable causing both to increase over time. These results suggest that tolerance and empathy are distinct constructs and may even oppose each other. For example, at Rutgers University in 2010, a young man (Dharun Ravi) expressed tolerance of his roommate (Tyler Clementi) being gay, but displayed a lack of empathy by filming Clementi's sexual encounter with another man, leading to Clementi's suicide (Foderaro 2010). This is an extreme example, but it illustrates that tolerance and empathy can be distinct.

Tolerance was also higher in years with more income inequality. This may be simply a product of both increasing in recent years, or it could be that the rise in individualism underlies both trends. By generation, tolerance was highest among Boomers, who experienced less income inequality when they were young. This may be one reason why Boomers broke new ground in tolerance while younger

generations such as GenX and Millennials continued the trend toward tolerance but did not break from the general time period trend.

If one generation always continues the trends of the previous generation, one might ask how social change ever occurs at all. In this case, the significant time period effects suggest that some Americans, perhaps primarily the first wave of Boomers, rejected their parents' lack of tolerance during adolescence or young adulthood and then raised their own children with these attitudes. This may have had a multiplier effect if tolerant parents always lead to tolerant children, but non-tolerant parents do not always lead to non-tolerant children. Thus, the time period and generational effects worked in tandem to produce a faster increase in Americans' tolerance for marginalized outgroups.

The increases in tolerance for gays and lesbians occurred at the same time that government policies (such as same-sex marriage and anti-discrimination laws that included sexual orientation) were beginning to change. Thus, as shown by Lax and Phillips (2009) when comparing variation among US states, voter opinion and state policies are fairly congruent. In other words, when public opinion shifts, so do government policies, at least around issues of tolerance for gays and lesbians.

The results could be interpreted as indicating a growing liberalization of attitudes among the US population, as tolerance for groups associated with liberal causes (homosexuals, Communists, atheists) has increased while tolerance for a non-liberal viewpoint (believing that Blacks are genetically inferior; Leonard [2005]) has increased to a smaller extent. However, tolerance for people with a belief in military rule, which is also not a traditionally liberal view, has also increased. In addition, self-identifying as a liberal or Democrat has decreased in the GSS since the 1970s, over the same time that tolerance increased. This suggests that the results are not due to the US population becoming more politically liberal. It is plausible that these views correlate with each other because they are associated with a more progressive rather than strictly liberal perspective, but the GSS does not separate liberal from progressive values.

Many of the effect sizes are moderate (around .50) to large (more than .80; Cohen [1988]; note that Cohen did not intend these values as cutoffs, but as a rough guide for interpretation). Tolerance for the civil liberties of controversial groups nearly doubled among many age groups; for example, Americans in their 60s were nearly a standard deviation more tolerant in the 2010s than their predecessors were in the 1970s (see table 1). Thus, the argument that time period and generational differences are too small to matter (Trzesniewski and Donnellan 2010) does not seem to apply to the attitudes and generations examined in the current paper. These larger differences in tolerance supplement other findings on the growth of individualism, such as the moderately sized changes in positive self-views (Twenge, Campbell, and Gentile 2012a).

Danigelis, Hardy, and Cutler (2007) examined these GSS items until 2004, focusing on intra-cohort aging. They concluded that people do not grow more conservative in their attitudes with age but instead grow more tolerant, with a surprising amount of change among people in their 60 s. The current analysis instead found that tolerance decreases with age when time period and generation are controlled. In addition, changes with time period and generation (those of the

same age over time) are much larger than changes with age and time period (following cohorts over time; see table 1). Thus, it seems likely that respondents in their 60 s did not show increased tolerance because they grew older, but because respondents in their 60 s underwent time period and generational shifts. Thus, our analyses suggest that the change observed by Danigelis, Hardy, and Cutler (2007) was not due to changes in attitudes during middle age, but to time period effects and generational replacement.

Limitations

One limitation of the current analysis is that the HLM coefficients were based on the available data. Those born in the 1920s and before were already in their 40 s and older when GSS data collection began in the 1970s. Similarly, as of 2012, those born in the 1980s had not yet reached their late 30 s or beyond, and those born in the 1950s had not yet reached their mid-60 s. Thus, it is possible that the apparent decline in tolerance with age may be partially due to generation, as Boomers, GenXers, and Millennials have not yet reached older ages. If the age trajectory of tolerance is different for these groups (and figure 3 suggests it might be), then future analyses incorporating more comprehensive life-span data may find that generation explains more of the increase in tolerance than suggested here.

Another limitation, as mentioned in the introduction, is that it is difficult to determine a precise role for individualism in changing tolerance. We provide some correlational data consistent with individualism, but the other mechanisms detailed earlier may also contribute (e.g., differential birth/mortality rates, parasite prevalence). Furthermore, these factors may interact in ways that we did not examine. Cultural change is complex, and while these data provide a clear picture of increasing tolerance and a relatively clear picture of the importance of a time period effect in this process, the role of individualism and other proposed mechanisms is primarily suggestive.

Finally, other variables may also contribute to increasing tolerance. For example, the communicability of stereotypes plays a role in their transmission (Schaller, Conway, and Tanchuk 2002). Perhaps the communication of negative stereotypes of and intolerance toward these groups has declined, resulting in increased tolerance. This possibility, however, cannot be addressed with the current data.

Conclusion

Overall, these results demonstrate considerable growth in the acceptance of the public expression of people with controversial beliefs or lifestyles. Americans are increasingly likely to believe that people with minority opinions—those substantially different from the larger group—have the right to speak, teach, and have their books in a community library. Both time period and generation are behind the increase in tolerance, possibly driven by concomitant increases in education and individualistic views rejecting traditional social rules. The increase in tolerance is consistent with an American culture that has become markedly more individualistic over the past few decades.

Appendix

Details of the HLM Analyses

We estimated a regression equation that tested linear, quadratic, and cubic terms to determine the trajectory of tolerance over the lifespan:

$$Y_{ijk} = \beta_{0jk} + \beta_{1jk}(\text{Age}) + \beta_{1jk}(\text{Age}^2) + \beta_{1jk}(\text{Age}^3). \quad (1)$$

This model takes into account the inherent nesting in cross-sectional data involving persons of different ages, nested within different birth year cohorts, which are in turn nested within the year the survey was conducted. We use the IRT-based estimate of the dependent variable, Y (i.e., total tolerance), for every person, i , in each birth year cohort, j , for every survey year, k .

The model in Equation 1 incorporates quadratic and cubic effects of age, denoted by Age^2 and Age^3 . To separate age effects from the effects of birth year cohort membership and the year the survey was conducted, a level-2 model was created that finds the unexplained variance in Y that is attributable to birth cohort and survey year. The level-2 model is stated:

$$\begin{aligned} \beta_{0jk} &= \pi_0 + t_{00j} + c_{00k} \\ \beta_{1jk} &= \pi_1 + t_{10j} + c_{10k} \\ \beta_{2jk} &= \pi_2 + t_{20j} + c_{20k} \\ \beta_{3jk} &= \pi_3 + c_{30k} \end{aligned} \quad (2)$$

The level-2 model treats each regression coefficient as an outcome variable. β_{0jk} is the mean of Y across birth cohorts and survey years at the average age, and this mean is partitioned into “rows” and “columns.” More specifically, the deviations from the mean β_{0jk} are taken across the “rows,” represented by t_{00j} , to estimate the effects for each birth year cohort. Effects away from β_{0jk} based on the survey year are represented by c_{00k} . Considerable effects for slopes, such as β_{1jk} , would be indicated by large variance components for t_{10k} and c_{10k} , and suggest that the linear slope of age in Equation 1 would differ by birth cohort or survey year, respectively. That is, for each coefficient, row and column effects on that coefficient are evaluated by the size of the variance component, σ^2 , one for each t and c coefficient in Equation 2. Notably, β_{3jk} is only predicted by c_{30k} , and t_{30j} is set to zero. This was done because including the weight variable caused the HLM7 program to require a parameter to be fixed. We chose to fix t_{30j} to zero because in unweighted analyses, this parameter had a near-zero ($< .001$) variance component (with $p > .50$).

About the Authors

Jean M. Twenge, Professor of Psychology at San Diego State University, is the author of more than 100 scientific publications and the book *Generation Me*:

Why Today's Young Americans Are More Confident, Assertive, Entitled—and More Miserable Than Ever Before and (2nd ed., 2014).

Nathan T. Carter is Assistant Professor of Psychology at the University of Georgia. His research interests include understanding the use of psychological measures in organizational settings, the accuracy of psychometric models, and the role of human judgment and decision-making in employee selection.

W. Keith Campbell is Professor and Head of the Department of Psychology at the University of Georgia. He is the author of more than 120 scientific publications, coauthor (with Jean M. Twenge) of *The Narcissism Epidemic: Living in the Age of Entitlement*, and coeditor (with Joshua D. Miller) of *The Handbook of Narcissism and Narcissistic Personality Disorder*.

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The Power of Love: The Role of Emotional Attributions and Standards in Heterosexuals' Attitudes toward Lesbian and Gay Couples

Long Doan, Lisa R. Miller, Annalise Loehr, *Indiana University*

Do people attribute emotions differently to members of various social groups? If so, do these differences have any bearing on formal and informal forms of social recognition? Using data from a nationally representative survey experiment, we examine whether American heterosexuals differentially attribute love to lesbian, gay, and heterosexual couples. We also examine the relationship between how in love lesbian, gay, and heterosexual couples are perceived to be and attitudes toward (1) granting them partnership benefits (formal rights); (2) the acceptability of their public displays of affection (informal privileges); and (3) marriage. Three main findings suggest that heterosexuals differentially attribute love to different types of romantic couples and that these differences are related to willingness to grant social recognition. First, gay couples are viewed as less loving than both heterosexual and lesbian couples; lesbian couples are seen as equally loving as heterosexual couples. Second, perceptions of love are related to willingness to grant social recognition. Third, perceptions of love matter more for gay and, to a lesser extent, lesbian couples than for heterosexual couples regarding informal privileges and marriage. In contrast, love matters equally for same-sex and heterosexual couples regarding formal rights. The results show that gay couples are penalized most in terms of perceptions of love and social recognition, whereas lesbians occupy a liminal space between heterosexual and gay couples. Collectively, these findings suggest that sexual identity and gender shape emotional attributions, which in turn play a key role in explaining inequalities that same-sex couples face.

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Attitudes toward lesbians and gays have become increasingly favorable over the past few decades (Loftus 2001; Powell et al. 2010). Despite this liberalization, recent data show that Americans continue to exhibit informal forms of prejudice, despite more willingness to grant formal, legal rights to same-sex couples (Doan, Loehr, and Miller 2014). In this paper, we extend this line of thinking by examining the role of emotional attributions, standards, and their differential application in explaining prejudice toward same-sex couples. In doing so, we investigate the ways in which same-sex relationships are potentially considered less legitimate and valid in a contemporary US context. Specifically, we examine gender and sexual identity differences in how loving lesbian, gay, and heterosexual couples are considered to be. We then ask whether differences in the attribution of love to romantic couples are, in turn, associated with the granting of social recognition to these couples.

Emotions scholars have long demonstrated that social norms and cultural standards play an important role in the experience and expression of emotions (Gordon 1990; Hochschild 1983; Thoits 1990). However, these cultural standards for emotions are complicated by stereotypes that lead to different attributions of emotions for members of different social groups (Barrett and Bliss-Moreau 2009; Tiedens, Ellsworth, and Mesquita 2000). Therefore, cultural stereotypes may color the extent to which members of different groups are seen as meeting emotional standards for a given situation. Given this premise, it is possible that similar expressions of love may be read differently based on the sexual identity of the couple. Furthermore, sexual identity and gender are intertwined and should be considered in tandem (Pascoe 2007). Thus, gendered emotion stereotypes may counteract sexual stereotypes for lesbian couples, while further penalizing gay male couples. A key question is whether people base their judgments of same-sex couples primarily on sexual identity, gender, or both. Regardless of the basis for differences in perceptions of same-sex couples' emotional displays, differential attributions of love may be associated with the granting of social recognition for these couples.

In this paper, we consider three areas of social recognition: partnership benefits (which conceptually belong to a larger set of items we call *formal rights*), public displays of affection (which conceptually belong to a larger set of items we call *informal privileges*), and the right to legally marry (which previous research has shown as having characteristics of both formal rights and informal privileges) (Cherlin 2004; Doan, Loehr, and Miller 2014; Powell 2013). Specifically, we ask the following research questions in this paper:

1. How does the *attribution* of love to a couple differ depending on the gender composition and sexual identity of the couple members (i.e., whether the couple is lesbian, gay, or heterosexual)?
2. Are perceptions of how loving a couple is linked with whether a couple is granted formal and informal forms of social recognition?
3. How does the relationship between perceptions of love and attitudes toward social recognition vary depending on couple type (lesbian, gay, or heterosexual)?
4. How do these patterns vary depending on the type of social recognition in question (i.e., formal rights, informal privileges, or marriage)?

We begin by reviewing sociological approaches to the study of love, research on how stereotypes affect emotional attributions, and research concerning the use of love as a standard by which Americans judge romantic couples. Combining these arguments, we derive hypotheses regarding the attribution of love to romantic couples, the relationship between such attributions and the granting of social recognition to the couple, and how this relationship varies by the gender composition and sexual identity of the couple members. Next, we distinguish between formal rights and informal privileges and review past findings regarding attitudes toward both. We then test our hypotheses using original data from a nationally representative survey experiment. Finally, we discuss the contributions of this study and implications for future research on the use of emotional standards to perpetuate social inequalities.

Background

Sociological Approaches to Love

The study of love has long been the subject of psychological and sociological attention. Although psychologists have written more on the topic than sociologists, sociologists too have systematically studied love, focusing on its broad, societal, cultural, and institutional patterns (Felmlee and Sprecher 2006). Structural studies of love observe how societies control love (Goode 1959), how sexual scripts provide guidelines for how social actors should behave and express themselves in romantic and sexual situations (Simon and Gagnon 1986), and how societal transformations shape the experience and expression of love in our culture (Giddens 1992). Various cultural studies have documented the often-conflicting components of love (Swidler 2001) and how definitions of love have changed (Cancian 1987). Others focus on the role of love in reinforcing women's subordination and diverting their energies from other achievements (de Beauvoir 1972). Across this diverse body of work, the motivation for the sociological study of love is to help explain the social and cultural context in which it is experienced (Jackson 1993). However, much of the existing sociological research is geared toward theory and philosophy more than empirical analysis (Felmlee and Sprecher 2006). We seek to fill this gap by empirically testing hypotheses derived from existing theories.

Although love is generally treated as an emotion in sociological research (Cancian and Gordon 1988; Hochschild 2003), scholars have not been able to reach a consensus in defining love. Our goal in this paper is not to resolve this ongoing debate. Rather, we treat love as a normative emotion for romantic couples in the United States. Indeed, we do not impose a definition of love in our empirical analyses. Instead, we allow respondents to use their own definitions and standards to evaluate the couple described in our study. In doing so, our conception of love should be consistent with how Americans generally define it. As sociologists have documented, the predominant cultural definition of love involves making a clear, unwavering choice to be exclusively committed to a partner (Swidler 2001), showing affection, and communicating about feelings. This view of love is similar to what Cancian (1987, 5) calls "feminized love,"

which refers to the way in which love is “identified with women and with qualities seen as feminine, such as tenderness and expressing feelings.” As we discuss later, this view of love leaves out instrumental, sexual, and more masculine notions, which may lead to the penalization of men in attributions of love. With this cultural definition in mind, we review differences in the attribution of love.

Differences in the Attribution of Love

Prior research has addressed the question of whether there are social group differences in the attribution of emotions. Indeed, research finds that stereotypes can lead to different attributions of emotions for members of various social groups even when they have similar expressive displays (Tiedens, Ellsworth, and Mesquita 2000). For instance, women are often stereotyped as being more emotional than men (Barrett and Bliss-Moreau 2009). Similarly, higher levels of anger are often attributed to African Americans compared to whites (Hugenberg and Bodenhausen 2004). Yet, a relatively unexplored area is sexual identity differences in the attribution of emotions. We consider whether heterosexuals differentially attribute love to lesbian, gay, and heterosexual couples. A key question is: Do people base their judgments of same-sex couples on sexual identity, gender, or both? Below, we consider each possibility in turn.

Regarding sexual identity, Risman and Schwartz (1988) find in their review of attitudes toward same-sex relationships that a prevailing stereotype is that lesbians and gays are less capable of having fulfilling, stable relationships than their heterosexual counterparts. In an early attempt to explore stereotypes regarding love for different couples, Testa, Kinder, and Ironson (1987) asked heterosexual college students how a hypothetical lesbian, gay, or heterosexual couple compared to other couples they know. They found that students rated both lesbian and gay couples as less loving and less satisfied than heterosexual couples. Since this early research on differences in *others'* perceptions of same-sex and heterosexual couples (but see Powell et al. [2010] for a recent exception), researchers have turned their attention to examining *self-reported* differences and similarities between same-sex and heterosexual couples (Gottman et al. 2003; Kurdek 2004; Weisshaar 2014).

In light of these findings and an increasing liberalization in attitudes toward homosexuality, whether stereotypes about lesbian and gay couples persist remains an empirical question in need of further research. If these stereotypes about lesbian and gay couples do prevail, this suggests that heterosexuals would believe that comparable same-sex couples are less loving than heterosexual couples:

Hypothesis 1a: *Heterosexuals view same-sex couples as less loving than heterosexual couples.*

Research has also shown gender differences in stereotypes regarding the emotionality of women and men, despite empirical evidence that contradicts these stereotypes (Simon and Nath 2004). In the arena of love, women are seen as “superior,” particularly because cultural conceptions of love are often feminized (Cancian 1987). Moreover, research shows that one of the feeling norms concerning romantic love for adolescent girls is that they should always be in love (Simon, Eder, and Evans 1992).

Indeed, dominant conceptions of love have typically excluded men's instrumental style of expressing love, such as providing help to others, sharing activities, and sexual intimacy (Cancian 1987). Research on sexual scripts shows that the dominant cultural norm is for men to focus on the physical aspects of relationships and for women to focus on the emotional aspects (Sakaluk et al. 2014; Simon and Gagnon 1986). Although sexual scripts in American culture are heterosexual scripts, research suggests that many same-sex couples adopt these scripts to some extent (Klinkenberg and Rose 1994). Given the cultural emphases on feminized love as well as differences in sexual scripts between men and women, adopting a gendered perspective of differences in the attribution of love suggests that lesbian relationships may be seen as most loving. This is because they involve two women, instead of just one in heterosexual relationships, or none in gay male relationships. Therefore:

Hypothesis 1b: Heterosexuals view lesbian couples as most loving, followed by heterosexual couples, then gay couples.

Finally, it is likely that when evaluating romantic couples, both sexuality and gender stereotypes activate because sexual identity and gender are intertwined. Adopting this view leads to similar predictions to the gendered perspective regarding evaluations of gay male couples. Gay men are likely to be seen as less loving than lesbians. However, because heterosexual romantic love is the ideal cultural image of romantic relationships (Wolkomir 2009), it is unlikely that lesbian relationships are evaluated more positively than this ideal. In other words, from this perspective, gender helps ameliorate the negative stereotypes against lesbian couples, but it does not eliminate them. Rather, gender serves as a counterbalance, buffering many of the negative effects that might result from sexual identity-based stereotypes about lesbians. This leads to another competing hypothesis:

Hypothesis 1c: Heterosexuals view gay couples as less loving, but lesbian couples as equally loving, compared to heterosexual couples.

Love and Legitimacy

There are compelling reasons to expect differences in the attribution of love to lesbian, gay, and heterosexual couples; these differences are also likely related to patterns of sexual prejudice. We contend that people use perceptions of love to gauge the legitimacy of a romantic couple, and that this emotional legitimacy is linked to people's willingness to grant social recognition to the couple. This argument is consistent with prior literature documenting how emotion plays a vital role in maintaining and reproducing inequality (Hochschild 1983; Lively 2000). Today, love-based unions are the predominant cultural norm in the United States (Coontz 2005). Indeed, empirical evidence also shows that people in the United States (Simpson, Campbell, and Berscheid 1986) and other Western countries (Levine et al. 1995) often consider love to be a prerequisite for long-term romantic relationships and marriage.

Yet, scholarship on love has not considered whether the attribution of this emotion varies based upon the sexual identity of a couple and whether these

differences might be linked with willingness to grant social recognition. Indeed, most emotions research has focused on the perspective of the person displaying an emotion rather than the perceiver of such displays. Research that has looked at how people perceive others' emotional displays has generally found that emotion displays are used to judge the character of an individual (Robinson, Smith-Lovin, and Tsoudis 1994). Recent theorizing by Shields (2005) takes this argument one step further to argue that appropriate emotional displays lend legitimacy to one's identities. Turner (2007) similarly argues that people maintain the flow of resources from others through the verification of their identities and emotions. This implies that Americans may withhold or grant social recognition based on the extent to which romantic couples appropriately perform their roles and display culturally expected emotions. Combined with our argument that people use perceptions of emotional legitimacy to gauge their willingness to grant social recognition to a romantic couple, this leads to our next hypothesis:

Hypothesis 2: Heterosexuals grant greater social recognition to couples perceived to be more in love.

Differences in the Effects of Love

In addition to differences in the attribution of love, it is possible that such attributions of love matter *more* for some couples than for others. Research on double standards and shifting standards has consistently demonstrated that disadvantaged groups face greater penalties for violating social norms and gain fewer rewards for demonstrating valued characteristics (Crawford and Popp 2003; Foschi 2000; Kreager and Staff 2009). Given the prevailing stereotype that lesbians and gays are less capable of achieving fulfilling, stable relationships than heterosexuals (Risman and Schwartz 1988), convincing people that a same-sex couple is as loving as a heterosexual couple may require greater evidence of romance. Alternatively, it could be that heterosexual couples simply do *not* have to demonstrate their love to be seen as deserving of social recognition. In either case, this suggests that same-sex couples would have to be seen as more loving in order to be deemed equally worthy of formal rights and informal privileges. Operationally, this means that the relationship between love and social recognition should be *greater* for same-sex couples than for heterosexual couples:

Hypothesis 3: The relationship between perceptions of love and overall social recognition of the couple is stronger for same-sex couples than for heterosexual couples.

Formal Rights, Informal Privileges, and Marriage

In this paper, we examine three forms of social recognition for romantic couples: formal rights, informal privileges, and marriage. *Formal rights* represent legal advantages that privileged groups often receive over disadvantaged groups. We examine attitudes toward partnership benefits as one measure of formal rights. In contrast to formal rights, *informal privileges* represent subtle, interactional advantages that privileged groups receive. These privileges are not legally

conferred, but arise in interactional settings as a way for the dominant group to maintain superiority over minority groups. Our measure for informal privileges is attitudes toward public displays of affection. *Marriage* is located between formal rights and informal privileges. Past research shows that although marriage is theoretically a formal right, it has symbolic aspects that are just as important as the legal and formal aspects (Cherlin 2004; Powell 2013). Furthermore, prior empirical evidence suggests that heterosexuals' attitudes toward marriage more closely mirror attitudes toward informal privileges than formal rights (Doan, Loehr, and Miller 2014).

Prior research underscores the need to examine formal rights and informal privileges as separate dimensions of attitudes toward homosexuality—an idea that is more broadly consistent with assertions that scholars must look at multiple dimensions in attitudinal research on prejudice more generally (Steeh and Krysan 1996). For this reason, we separately examine how the attribution of love is linked with Americans' willingness to grant formal rights, informal privileges, and marriage. In doing so, we are able to examine potential differences in the relationship between perceptions of love and the willingness to grant social recognition.

Given the limited amount of existing research on which to base theoretically derived hypotheses, we do not offer formal hypotheses concerning potential differences in the relationship between love and these forms of social recognition. Existing research suggests that perceptions of love are linked with approval of formal rights and marriage in the gay rights movement (Moscowitz 2013), but there have not yet been empirically documented linkages between love and informal privileges. This might suggest that perceptions of love may be linked to formal rights and marriage, but not to informal privileges. How the relationships between perceptions of love and formal rights, informal privileges, and marriage may differ is an open empirical question we examine in the analyses.

Methods

Data

To test our hypotheses, we use a nationally representative data set collected by GfK (formerly Knowledge Networks) with support from Time-Sharing Experiments for the Social Sciences (TESS) (Doan, Loehr, and Miller 2014; Freese and Visser 2010).¹ Participants are a random subset of the GfK panel, a nationally representative online panel of US residents recruited using a combination of list-assisted random-digit-dial and address-based sampling methods. The study was fielded in December 2010. Because the core arguments of the paper are about heterosexuals' attitudes toward same-sex and heterosexual couples, we restrict our analyses to the 505 heterosexuals in the data set. Nineteen respondents did not answer at least one of the dependent variables, yielding an analytic sample size of 486. The completion rate (percentage of people who are asked to participate in the study who completed it) for the study is 63.5 percent, comparable to other studies using TESS data. The completion rate for heterosexuals is 62.4 percent. Note, however, that this completion rate is different from response rates

often reported in phone and mail surveys because respondents are recruited from a panel of people who have already committed to participating in a certain number of surveys.

Design

Respondents are randomly assigned to one of three vignette conditions and then asked to answer questions about a hypothetical couple described in the vignette. The names and personal pronouns used in the vignettes are varied to manipulate the couple's sexual identity. The heterosexual couple vignette, for example, reads:

Brian and Jennifer met three years ago and were immediately attracted to each other. After going on a few dates, Brian told Jennifer that he wanted to see her exclusively, to which Jennifer happily agreed; they've been together ever since. Although Brian and Jennifer have had serious arguments, they both report being happy when they are together. Brian and Jennifer feel complete trust in each other. In fact, they've lived together for the past two years.

"Brian and Jennifer" and their pronouns are replaced with "Heather and Jennifer" or "Brian and Matt" in the same-sex couple conditions.²

Measures

After reading the vignette, respondents are asked to assess how in love the couple seems. *How in love* is measured with the following question: "How in love would you say [couple members' names] are, if 0 means that they are not at all in love and 10 means that they are completely in love?" This item is modeled after the General Social Survey's intensity of anger question in the 1996 emotions module (Davis and Smith 2009).³

Next, respondents are asked questions about whether the vignette couple should be granted formal rights, informal privileges, and be allowed to legally marry.

Formal rights

The formal rights item is a summative scale ($\alpha = .86$) of partnership benefits, such as *family leave*, *hospital visitation*, *inheritance rights*, and *insurance benefits*. Respondents are asked to what extent they agree or disagree with the vignette couple having each partnership benefit (coded 0 = strongly disagree to 3 = strongly agree). The summed scale thus ranges from 0 to 12, representing strong disagreement to all items to strong agreement to all items.

Informal privileges

Similarly, the informal privileges item is a summative scale ($\alpha = .87$, also ranging from 0 to 12) of the extent to which respondents think it is acceptable for the vignette couple to *tell others* that they are in a relationship, *hold hands*, *kiss on the cheek*, and *French-kiss* in a park.

Marriage

Marriage is a binary variable that compares respondents who agree (somewhat or strongly) that the couple should be allowed to legally marry to those who disagree (somewhat or strongly).⁴

Independent Variables

We include indicator variables for the vignette condition that the respondent received. *Lesbian couple* is an indicator variable where 1 means that the respondent received the lesbian couple vignette, and *gay couple* is an indicator variable where 1 means that the respondent received the gay couple vignette. The reference category is the heterosexual couple vignette. Descriptive statistics and definitions for these variables are found in table 1.

Table 1 also breaks down the descriptive statistics for each item by vignette condition. Some differences in our variables of interest are apparent here. We more formally examine these differences in our results. The sociodemographic variables are roughly similar across condition, as they should be, given that the conditions are randomly assigned. To account for residual bias, we include controls for sociodemographic factors such as age (Avery et al. 2007), race (Lewis 2003), gender (Meaney and Rye 2010), marital status (Kunkel and Temple 1992), children (Herek and Gonzalez-Rivera 2006), education (Kozloski 2010), income (Andersen and Fetner 2008), political affiliation (Schwartz 2010), and religion (Olson, Cadge, and Harrison 2006), all of which have been shown to influence attitudes toward homosexuality.

Analytic Strategy

We begin by examining whether there are differences in the attribution of love among heterosexual, lesbian, and gay couples.⁵ To assess whether love is a standard by which couples are judged, we then estimate models regressing formal rights, informal privileges, and marriage on love. We next examine the mediating role of perceived love using a Sobel (1982) test of mediation for the OLS models and the KHB decomposition method (Breen, Karlson, and Holm 2013) to test for mediation in the binary logistic regression models.⁶ Because we are interested primarily in the differences in the attribution of love as a dependent variable by couple type, we did not experimentally manipulate the love variable in the current study. Therefore, while we are able to show that the patterns observed are at least *consistent* with our theoretical argument, we are more cautious in our interpretations of the effects of love on approval of social recognition than in the effects of sexual identity, which we did experimentally manipulate. However, existing theory and empirical patterns more strongly support the assertion that the perception of love is an emotional standard.

Finally, we examine the relationship between attributed love and social recognition by vignette condition. Here, we include product terms between how in love and vignette condition to examine if the effect of love varies by condition. For the ordinary least squares (OLS) models, significant product terms would tell us that love matters differently for lesbian, gay, and heterosexual couples. Examining interaction effects for the binary logistic regression models are more complicated, as we will show later in our discussion of the approval of legal marriage.

Table 1. Descriptive Statistics for Dependent and Independent Variables

Name	Item description	Full sample		Heterosexual vignette		Gay vignette		Lesbian vignette	
		Mean/ Percent	S.D.	Mean/ Percent	S.D.	Mean/ Percent	S.D.	Mean/ Percent	S.D.
Key variables									
How in love	Level of love the respondent attributes to the couple (0–10; Not at all in love to completely in love)	6.512	2.347	6.809	1.909	5.994	2.609	6.710	2.433
Formal rights	Summative scale of approval of formal rights items (0–12)	7.840	3.263	7.803	2.795	7.759	3.563	7.961	3.441
Informal privileges	Summative scale of approval of informal privileges items (0–12)	7.331	3.404	9.382	1.912	5.614	3.576	6.794	3.367
Marry	Respondent approves of legal marriage (1 = Yes)	71.19%	–	98.84%	–	53.80%	–	58.06%	–
Control variables									
Female (vs. male)	Female respondent (1 = Yes)	47.33%	–	50.29%	–	48.73%	–	42.58%	–
Age	Respondent's age (18–91)	49.619	16.148	49.601	16.484	50.310	15.963	48.935	16.030
White (vs. non-white)	White respondent (1 = Yes)	76.13%	–	78.03%	–	77.22%	–	72.90%	–
College or more	Respondent's highest degree received is at least a bachelor's degree (1 = Yes)	30.66%	–	31.21%	–	29.75%	–	30.97%	–
High school	Respondent's highest degree received is a high school diploma (1 = Yes)	59.05%	–	60.12%	–	58.23%	–	58.71%	–

Less than high school (reference category)	Respondent's highest degree received is less than a high school diploma (1 = Yes)	10.29%	-	8.67%	-	12.03%	-	10.32%	-
At least \$50,000 (vs. less than \$50,000)	Respondent's household income is at least \$50,000 a year (1 = Yes)	48.35%	-	49.13%	-	46.84%	-	49.03%	-
Married (vs. unmarried)	Respondent is married or living with a partner (1 = Yes)	58.44%	-	50.87%	-	65.82%	-	59.35%	-
Children (vs. no children)	There is at least one child living with the respondent (1 = Yes)	29.84%	-	26.59%	-	31.65%	-	31.61%	-
Number of children	The number of children under 18 living with the respondent (0-5)	0.535	0.983	0.532	1.070	0.563	0.967	0.510	0.900
Political ideology	Political ideology (1-7; Extremely liberal to extremely conservative)	4.235	1.437	4.191	1.407	4.146	1.391	4.374	1.512
Evangelical	Evangelical respondent (1 = Yes)	30.45%	-	28.32%	-	29.11%	-	34.19%	-
Mainline	Mainline Protestant respondent (1 = Yes)	18.93%	-	17.92%	-	18.35%	-	20.65%	-
Black Protestant	Black Protestant respondent (1 = Yes)	7.20%	-	7.51%	-	6.33%	-	7.74%	-
Catholic	Catholic respondent (1 = Yes)	20.99%	-	24.28%	-	18.99%	-	19.35%	-
Other affiliation	Respondent reports a religious affiliation other than Evangelical, Mainline Protestant, Black Protestant, or Catholic (1 = Yes)	6.38%	-	6.36%	-	8.86%	-	3.87%	-
No affiliation (reference category)	Respondent reports no religious affiliation (1 = Yes)	16.05%	-	15.61%	-	18.35%	-	14.19%	-
N	Number of cases in each vignette condition	486	173	158	155				

As Ai and Norton (2003, 129) note, “The interaction effect...cannot be evaluated simply by looking at the sign, magnitude, or statistical significance of the coefficient on the interaction term when the model is nonlinear.” Therefore, following current methodological recommendations, we test for the interaction effect in our models examining marriage by computing the second difference in the effect of love and testing whether this difference is significantly different from zero at meaningful values (Ai and Norton 2003; Berry, DeMerritt, and Esarey 2010). For all models, we include coefficients for variables of interest in the main text and full tables of coefficients with controls in the online supplemental appendix.

Results

Differences in the Attribution of Love

Table 2 includes coefficients from OLS regressions examining the effects of vignette condition on the attribution of love. These results allow us to answer our main research question and test whether and how heterosexuals differentially attribute love to same-sex couples.⁷ As shown by model 1 in table 2, we find a significant and negative effect of the gay vignette condition compared to the heterosexual vignette condition. In other words, gay couples are seen as less loving than heterosexual couples. The gay vignette coefficient is also significantly smaller than the lesbian vignette coefficient ($F = 7.43, p < .01$), suggesting that gay couples are seen as less loving than lesbian couples as well. The differences between gay couples and both heterosexual and lesbian couples are not diminished with the inclusion of sociodemographic controls.

Although heterosexuals attribute less love to gay couples, they do not significantly differentiate between lesbian couples and heterosexual couples. In other words, lesbian couples are viewed as just as loving as heterosexual couples. In answer to our first major research question, we find support for Hypothesis 1c, which predicts that heterosexuals will rate gay couples, but not lesbian couples, as less loving than heterosexual couples. The results suggest that heterosexuals differentially attribute love to heterosexual and lesbian couples compared to gay couples, which means that *both* sexual identity and gender stereotypes are

Table 2. Unstandardized Coefficients of OLS Regressions of Perceived Level of Love on Vignette Condition

	Without controls	With controls ^a
Gay vignette	-.816 ^{**b} (.256)	-.817 ^{***b} (.246)
Lesbian vignette	-.100 (.257)	-.008 (.247)

Note: $N = 486$. Standard errors in parentheses.

^{**} $p < .01$ ^{***} $p < .001$ (two-tailed tests)

^aIncludes controls for sex, age, race, education, household income, marital status, presence and number of children, political ideology, and religion.

^bGay vignette and lesbian vignette coefficients are significantly different.

important in attributions of love. Furthermore, because only gay couples are penalized when it comes to perceptions of love, perceived love can only be a mediator for gay couples. In other words, there may be differences in approval of social recognition for lesbian and heterosexual couples, but only differences between the gay and heterosexual couples can be attributable to differences in perceptions of love.

Love as Emotional Legitimacy

Next, we examine whether differences in the attribution of love to heterosexual and lesbian couples compared to gay couples affect the willingness to grant formal rights, informal privileges, and marital rights. Table 3 includes results from OLS regressions of formal rights and informal privileges and binary logistic regressions of approval of marriage on levels of attributed love. Across these items (see main effects models), we find that higher attributions of love have a positive and significant effect on approval for all items. In other words, the more loving a couple is perceived to be, the more that couple is seen as deserving of formal rights, informal privileges, and legal marriage. On the whole, these results provide strong support for Hypothesis 2, which predicts that couples who are perceived as more loving are significantly more likely to be granted formal rights, informal privileges, and marriage.

Next, we examine the mediating effect of love. Baron and Kenny (1986) identify the conditions under which a variable is considered a mediator. The Sobel (1982) test provides a composite measure of these conditions and associated significance test of whether a variable mediates the effect of another variable. The KHB method (Breen, Karlson, and Holm 2013) extends the Sobel test to logistic regressions. As shown in table S1 in the supplementary materials online, there are significant differences by condition for informal privileges and marriage. Combined with the results from table 2 above, love may be a mediator for the differences between the gay vignette and the heterosexual vignette. Therefore, we test for this possibility.

For informal privileges, perception of love is a significant mediator for the gay vignette. Perception of love mediates 15 percent of the direct effect of condition on willingness to grant informal privileges ($p < .01$). Similarly, perception of love also mediates 7 percent of the direct effect of condition on willingness to grant legal marriage ($p < .05$). In both cases, love is a partial mediator, explaining a significant proportion, but not the majority, of the difference between conditions. Indeed, although the direct effect of the gay vignette is reduced with the inclusion of the love variable, there remains a significant effect of condition. This suggests that perceptions of love help explain prejudice against gay couples when it comes to informal privileges and marriage, but it is not the sole explanation for prejudice against gay couples.

Differences in the Effects of Love

In a previous section, we have shown that heterosexuals perceive gay male couples as less loving than heterosexual couples. Additionally, we found that couples perceived as more loving are also more likely to be granted formal rights, informal privileges, and legal marriage. We now examine sexual identity differences,

Table 3. Estimates from Regressions Showing the Conditional Effect of Love on Formal Rights, Informal Privileges, and Marriage

	Formal rights ^a			Informal privileges ^a			Marriage ^b	
	Main effects	Interaction effects	With controls ^c	Main effects	Interaction effects	With controls ^c	Main effects	Interaction effects
Gay vignette	.528 (.315)	-.798 (.980)	-.417 (.947)	-3.199*** (.283)	-5.245*** (.875)	-5.300*** (.852)	-4.277*** (.739)	-5.936** (2.207)
Lesbian vignette	.228 (.314)	-1.499 (1.043)	-.375 (1.009)	-2.519*** (.281)	-3.547*** (.932)	-3.222*** (.908)	-4.369*** (.743)	-4.769* (2.203)
How in love	.701*** (.056)	.531*** (.113)	.491*** (.110)	.697*** (.050)	.515*** (.101)	.399*** (.099)	.383*** (.057)	.201 (.334)
Gay vignette × How in love		.198 (.143)	.118 (.138)		.317* (.127)	.315* (.124)		.288 (.346)
Lesbian vignette × How in love		.255 (.147)	.114 (.142)		.151 (.131)	.123 (.128)		.090 (.343)

Note: *N* = 486. Standard errors in parentheses.

p* < .05 *p* < .01 ****p* < .001 (two-tailed tests)

^aOLS regressions.

^bBinary logistic regressions.

^cIncludes controls for sex, age, race, education, household income, marital status, presence and number of children, political ideology, and religion.

^dGay vignette and lesbian vignette coefficients are significantly different.

paying specific attention to how these differences translate to support for social recognition for lesbian and gay couples. To do so, we examine the effect of love on support by condition in the interaction effects model in table 3. The final model adds sociodemographic controls.

Starting with the formal rights models, we find a significant main effect, but no significant interaction effects of love by couple type. After taking into account sociodemographic differences, the main effect for love on formal rights remains significant ($p < .001$). Combined with the nonsignificant and small magnitude of effect for the interaction term, this suggests that the effect of love on formal rights is constant among different couple types. Thus, love matters equally for same-sex and heterosexual couples when it comes to approval of formal rights. Indeed, there is no significant difference in the effect of love on formal rights for same-sex and heterosexual couples.

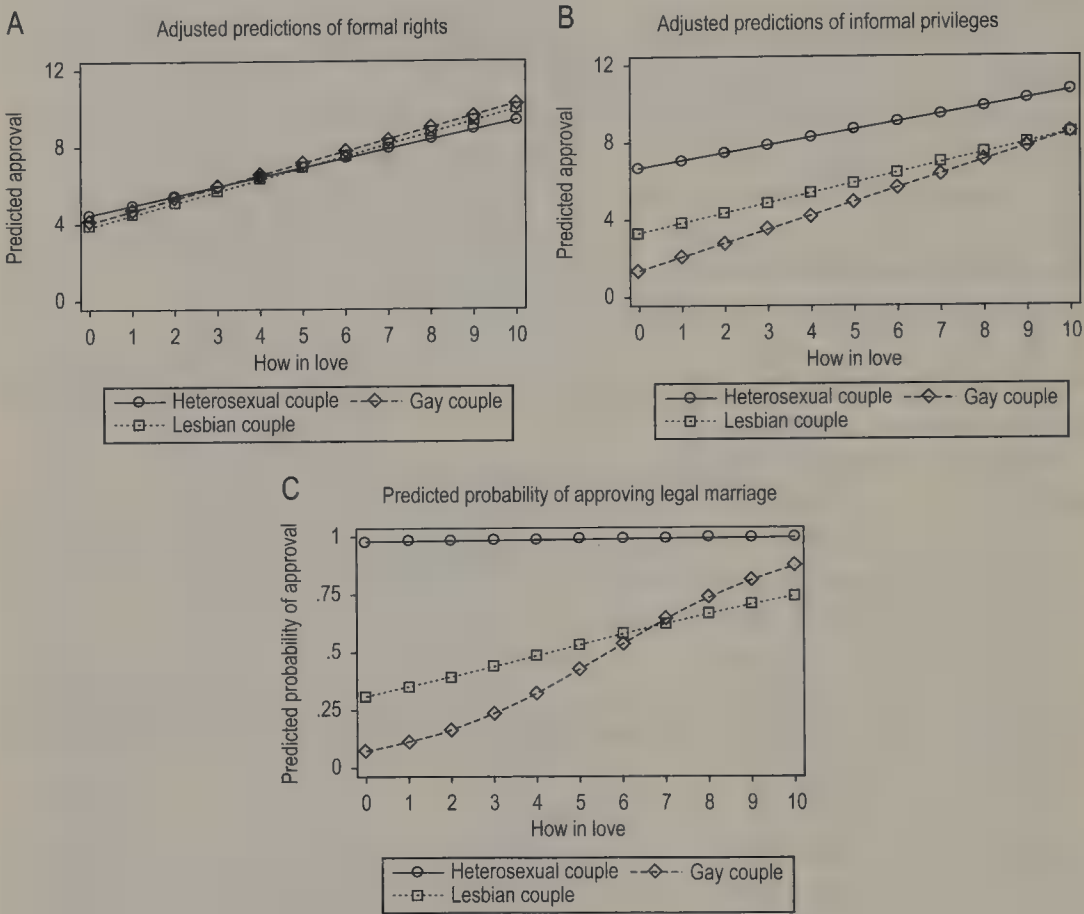
The story is different regarding informal privileges. Here, we find significant main effects as well as interaction effects for levels of love. A unit increase in the level of the love attributed to all couples is associated with a .515 increase in the informal privileges scale for all couples ($p < .001$). However, this effect is stronger for the gay couple (a further increase of .317, $p < .05$). Interestingly, the interaction term for the lesbian couple is statistically nonsignificant and small in magnitude, suggesting that lesbians are judged on par with heterosexuals when it comes to love. With the inclusion of sociodemographic controls, the interaction between level of love and condition remains similar in both magnitude and significance.

Finally, we examine the effect of love on approval of legal marriage for the couple. Here, we find significant penalization for both lesbian and gay couples ($p < .001$). Indeed, heterosexuals have 100 percent lower odds of approving marriage for the gay couple than for the heterosexual couple ($e^{-5.936} - 1 = -.997$). As shown in table 3, both the main effect of love and the product terms between love and condition are statistically nonsignificant. However, recall that nonsignificance in the log-odds does not necessarily mean that love has no effect on the probability of approving marriage (Ai and Norton 2003; Berry, DeMeritt, and Esarey 2010). To examine the effect of love on the *probability* of approving of marriage, we rely on the change in predicted probability by condition (marginal effect), and the rate of this change by condition (second difference of the marginal effect).

For respondents with mean values on all sociodemographic values, the “average” respondent, the marginal effect of love on their approval of marriage for the heterosexual couple is .001, statistically indistinguishable from zero. The marginal effect is .111 for the gay couple ($p < .001$) and .044 for the lesbian couple ($p = .050$). When comparing the second difference in the effect of love, we find a significant difference between the gay couple and the heterosexual couples ($p < .001$) and a marginally significant difference between the lesbian couple and the heterosexual couples ($p = .056$). In other words, for the “average” respondent, love has a negligible effect on whether the respondent thinks a heterosexual couple should be allowed to legally wed. Love plays a significantly larger role in this decision for the lesbian and gay couples.⁸

Figure 1 demonstrates the differences in the effect of love for each outcome. In panel A, we see the constant effect of love for all couple types when it comes to

Figure 1. The effect of love by condition



formal rights. Panel B shows the stronger effect of love for gay couples, but not lesbian couples. Panel C shows the predicted probability of approving marriage for the heterosexual, gay, and lesbian couples across the range of the love item with all sociodemographic controls held at their means. As shown in panel C, heterosexuals are generally approving of heterosexual marriage, regardless of how in love they perceive the couple to be. The predicted probability of approving of marriage for a heterosexual couple that is “not at all in love” is .98. This probability increases to 1 for a heterosexual couple who is “completely in love.” There is much more movement for the lesbian and gay couples. The predicted probability of approving of marriage for gay couples starts out as .07 when “not at all in love,” and moves to .87 when “completely in love.” The predicted probability for lesbian couples starts at .31 and moves to .74. In other words, a same-sex couple that is seen as having the highest level of love has a lower predicted probability of approval of marriage than a heterosexual couple that is seen as not at all loving. Nevertheless, the change in predicted probability for the lesbian and gay couples across the range of the love item is large compared to the change in predicted probability for the heterosexual couple, suggesting a double standard.

Overall, we find mixed support for Hypothesis 3, which predicts that love will matter *more* for same-sex couples than heterosexual couples. While we find that love does indeed matter more for approval of informal privileges and marriage

for gay couples compared to heterosexual couples, we also find that love matters equally for formal rights among all couples. We find strong evidence of this double standard for gay couples and little evidence of a double standard for lesbian couples (although we do find marginal significance for the lesbian couple when it comes to marriage).

Discussion

In this paper, we examine love as an emotional standard by which romantic couples are judged. Using underutilized arguments in the sociology of emotions literature regarding the rewards of conforming to emotion norms, we link emotional attributions to the granting of social recognition. Not only do people reward others for conforming to emotion norms, but the stakes are higher for some groups. Using data from a nationally representative survey experiment, we test hypotheses derived from these arguments.

Our analyses reveal three major findings. First and foremost, we find that gay couples are at a disadvantage regarding love. They are deemed as less loving than both heterosexual and lesbian couples. Contrary to early research (Testa, Kinder, and Ironson 1987), lesbian couples do not face the same penalty and are deemed as equally loving compared to heterosexual couples. Second, we find that perceptions of love are related to willingness to grant social recognition to romantic couples of all types. Indeed, perceptions of love partially mediate the relationship between receiving the gay couple vignette and less approving attitudes toward informal privileges and marriage. Third, we find that not only are gay couples seen as less loving, the effect of love for gay couples is greater than the effect of love for heterosexual couples. Perceptions of lesbian couples do not follow the same pattern. Although we find differences between lesbian and heterosexual couples in terms of approval of informal privileges and marriage, these differences are not as linked to perceptions of love as they are for gay couples. Together, these findings have theoretical implications for the intersecting nature of social identities, the sociology of emotions, and the study of prejudice based on sexual identity, as well as practical implications for the LGBT movement.

Regarding the overlapping nature of social identities, we examined different hypotheses surrounding the attribution of love and asked whether judgments are made based upon sexual identity, gender, or both. Consistent with the idea that *both* sexual identity and gender stereotypes activate in people's minds, we find that lesbian couples are seen as more loving than gay couples and just as loving as heterosexual couples; in other words, for lesbians, gender and sexual identity stereotypes may cancel out each other's effects. The same argument would explain why gay couples, being sexual minorities as well as men, are doubly penalized in perceptions of their love. Because love is often seen in feminized terms (Cancian 1987), gay men may be penalized by Americans who overlook instrumental styles of love. Indeed, gay relationships, but not lesbian relationships, are seen as more physical and sexual in nature than heterosexual relationships (Stewart 1999). Although we do not have the data to fully test these arguments, these results suggest that teasing out the oft-confounded effects of sex, sexuality, and gender is a fruitful endeavor for future research (Mize 2014).

The results suggest that lesbians occupy a liminal space between heterosexuals and gay men, highlighting the importance of the role of gender in shaping attitudes toward sexual identity. Scholars have suggested that homophobia and masculinity operate in conjunction with each other (Pascoe 2007), and our findings raise important questions about whether emotional stereotypes associated with femininity sometimes work to advantage lesbian women in relation to gay men. Consistent with recent research that shows that negative stereotypes about gay men can ironically have positive consequences for Black gay men insofar as they counteract negative racial stereotypes (Pedulla 2014), we call attention to the intersecting nature of social identities (i.e., gender and sexuality) in shaping attitudes toward gays and lesbians. Our results also suggest that it is important to separate lesbians and gays in research on prejudice based on sexual identity (see Herek 2000), despite a tendency in past work to analyze them together.

Our study also makes several contributions to the sociology of emotions. Although the transactional rewards of appropriate emotional displays (Turner 2007) have been theorized, little empirical work tests these ideas. We add to the body of work that shows that people use emotional attributions in determinations of whether individuals are deserving of certain forms of social recognition (e.g., Robinson, Smith-Lovin, and Tsoudis 1994). In addition to highlighting the role that emotional standards play in the granting of social recognition, our findings also highlight that studying the perspective of the perceiver can shed light on how emotions may be used as subtle justification for perpetuating inequality. Indeed, prior research on emotion management processes similarly finds that a failure to conform to gendered and racialized feeling norms of anger can result in women and African Americans losing status and facing detrimental professional consequences (Brescoll and Uhlmann 2008; Pierce 1995; Wingfield 2010). Yet, far less attention is paid to how people's emotional attributions affect assessments of whether others should receive social recognition (but see Robinson, Smith-Lovin, and Tsoudis 1994). This line of research merits further exploration, as the attribution of emotion to marginalized groups can have detrimental consequences in the workplace, political sphere, and society, not only for women and racial minorities (Simon and Nath 2004; Wingfield 2010), but also for sexual minorities.

Regarding prejudice based on sexual identity, the linkage between perceptions of love and willingness to grant formal and informal forms of social recognition suggests that justifications for prejudice based on sexual identity may be becoming subtler. Indeed, not having one's relationship validated and seen as loving is itself a subtle justification for prejudice. These findings are consistent with the idea of heteronormativity, where same-sex relationships are viewed as less legitimate than heterosexual relationships (Warner 1993). They also mirror research on modern prejudice, which has demonstrated the shift from blatant forms of prejudice to more subtle forms (Bonilla-Silva 2001; Doan, Loehr, and Miller 2014; Swim et al. 1995). Consistent with this shift, scholars argue that Americans must continue to find ways to justify inequality on more subtle grounds (Bobo 1999; Jackman 1994). As our study demonstrates, emotional attributions may be an important subtle justification for perpetuating informal inequalities based on sexual identity.

The findings also highlight the difference between formal rights and informal privileges, not only in terms of attitudes toward them, but also in how people seem to approach evaluating each. We find that love matters more for gay male couples for informal privileges, but not for formal rights. One possibility for this difference is that attitudes are multidimensional and the cognitive bases for informal privileges and formal rights are different (Doan, Loehr, and Miller 2014). Thus, formal rights-based questions likely activate values and beliefs about egalitarianism, which elicit more equal standards than those that do not (Loftus 2001). However, items concerning general social acceptance like informal privileges are more prone to subtle prejudice. This may be why we find evidence of a double standard for informal privileges, but not for formal rights. Indeed, recent research suggests that different “evaluative mind-sets” lead to differences in the extent to which double standards are applied (Biernat, Fuegen, and Kobryniewicz 2010).

Practically speaking, the finding that love matters more for gay male couples presents a double bind for the LGBT movement. Our findings suggest that the LGBT movement’s focus on the portrayal of the loving couple in an attempt to gain rights is fruitful, especially given the finding of equal standards for formal rights. However, one of the primary ways to convince others of one’s love is to display affection, which heterosexuals are significantly less approving of same-sex couples doing. Past research examining the LGBT movement finds concern about this strategy among movement leaders, precisely because of this double bind (Moscowitz 2013; Nussbaum 2009).

Limitations and Future Directions

Several data limitations lend caution to our interpretations of findings. Although we are able to speak to the causal ordering of sexual identity and attributions of love because we experimentally manipulated sexual identity, we are less certain about the causal ordering of love and approval of formal rights and informal privileges. However, if the reverse causal ordering is correct (i.e., formal rights and informal privileges predict love), we would expect more differences between attributed love between same-sex and heterosexual couples than observed. This is because we find clear differences in heterosexuals’ approval of informal privileges between lesbian couples and heterosexual couples, but no difference in the attributions of love between lesbian couples and heterosexual couples. Our mediation analyses also highlight the role of love as a mediating variable.

It is also possible that an unmeasured third factor, such as general prejudice toward same-sex couples, may be driving both the attribution of love and attitudes toward social recognition. However, if general prejudice toward same-sex couples drives both relationships, we might expect great polarization in attitudes (i.e., a large proportion of people who pick either extreme for *both* love and recognition). This is not empirically the case, as only 1 to 7 percent of respondents are in these extreme categories. Future research could address this limitation by manipulating the displays of love for the couples and, in turn, also examine the use of double standards at different levels of the standard. As scholars of mental health have argued, theories that rely on cultural standards are most powerful in

light of ambiguity (Pescosolido, McLeod, and Alegría 2000). In other words, there is less room for double standards when there is clear evidence that couples are not in love or clear evidence that they are completely in love. We have created a couple that is on average rated at about the middle of the possible range for love (rated about 6 or 7 on the 11-point scale). Our findings may differ for couples that are clearly seen as not loving or very loving.

Despite these limitations, we are able to show that the patterns observed are at least *consistent* with our theoretical argument. Furthermore, because we experimentally manipulated the sexual identity of the couples in our vignettes, we can speak to the causal effect of sexual identity on attributions of love. Indeed, this study is the first of its kind to use a nationally representative data set to examine the relationship between sexual identity, attributions of love, and prejudice. As such, this study makes several contributions to the sociology of emotions and the study of prejudice based on sexual identity, and opens new lines of inquiry worth further pursuit.

It is also one of the few studies that explicitly examines the use of emotional standards by which people judge others. This avenue allows for further exploration of norms regarding the expression and experience of emotions and their use to the advantage or detriment of certain groups within society. Finally, despite suggesting some double standards of love for public displays of affection and marriage, our study also provides evidence that equal standards of love exist for the granting of formal rights. As a result, the LGBT movement's focus on the portrayal of the loving, committed couple is likely a fruitful approach to gaining rights (Hatalsky 2011). Scholars and activists alike should not underestimate the power of people's perceptions of love; such perceptions of love's presence or absence can become an influential force for change, or the basis for resistance to such change.

Notes

1. TESS conducts population-based experiments using the GfK panel with support from the National Science Foundation. See the GfK (<http://www.knowledgenetworks.com>) and TESS (<http://www.tessexperiments.org>) websites for more information.
2. We designed the names to reduce any confounding effects due to perceived age and racial differences between the partners. Further, to reduce the possibility of the order of the names confounding the results, the first person asks out the second person in each vignette. In doing so, we can more directly examine the effects of sexual identity.
3. The GSS question reads "How intense was your anger?" with 0 labeled as "not at all intense" and 10 labeled as "very intense."
4. In supplemental analyses, we also examined marriage as part of the informal privileges scale because factor analyses in Doan, Loehr, and Miller (2014) found that heterosexuals tend to treat marriage more similarly to informal privileges. Additionally, leaving the item as an ordinal variable and using ordered logistic regressions leads to similar conclusions. Dichotomizing the variable simplifies the presentation of the results.
5. We present unweighted results in this paper. However, results that incorporate survey weights are substantively similar. Furthermore, models using indices of the number of items endorsed as well as factor scores from exploratory factor analyses are substantively similar to the more parsimonious models presented here.

6. A Sobel test is not appropriate for the logit because, unlike in OLS models, the inclusion of a mediator variable in the logit will alter the coefficient of the independent variable regardless of whether it is actually a mediator, as long as the mediator is correlated with the outcome. The KHB method rescales the variables to account for this problem in calculating direct and indirect effects. We also calculated indirect effects using standardized variables and bootstrapping standard errors (Winship and Mare 1984; Long 1997), and the conclusions are the same.
7. Supplemental analyses looking at lesbians and gays show that sexual minorities do not differentiate among couple types in their attributions of love compared to heterosexuals. Lesbians and gays do not significantly differ from heterosexuals when examining the relationship between love and social resources for formal rights and informal privileges. There was not enough variation in the lesbian and gay subsample in terms of attitudes toward marriage to perform these supplemental analyses on that item.
8. This difference is similar when we look at the average marginal effect (i.e., calculating the marginal effect for everyone in the sample and then averaging these effects) instead of the marginal effect at the mean. Full results from these two analyses can be found in online supplementary table S2.

About the Authors

Long Doan is a PhD candidate in sociology at Indiana University. He is broadly interested in how emotions motivate behavior and maintain inequality. His dissertation examines the ways in which people's gender and race jointly affect others' interpretations of their emotion displays and, in turn, subsequent evaluations of them. His publications have appeared in the *American Sociological Review*, *Archives of Sexual Behavior*, and *Social Psychology Quarterly*.

Lisa R. Miller is a PhD candidate in the Department of Sociology at Indiana University. She specializes in the sociology of gender, aging and the life course, and sexualities. Her recent research investigates the nature and consequences of prejudice and discrimination against LGBTQ individuals. Her dissertation examines how women navigate dating and sexual partnerships throughout the life course. Her recent publications appear in the *American Sociological Review*, *Sociological Forum*, and *Archives of Sexual Behavior*.

Annalise Loehr is a PhD student in the Department of Sociology at Indiana University. Her primary areas of interest include social psychology, gender, sexualities, deviance, and the scholarship of teaching and learning. She has coauthored publications in the *American Sociological Review*, *Archives of Sexual Behavior*, and *Teaching Sociology*. She is currently teaching abroad at the University of Mannheim in Baden-Württemberg, Germany.

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Network Intervention: Assessing the Effects of Formal Mentoring on Workplace Networks

Sameer B. Srivastava, *University of California–Berkeley*

This article assesses the effects of formal mentoring on workplace networks. It also provides conceptual clarity and empirical evidence on expected gender differences in the effects of such programs. Qualitative interviews with 40 past participants in a formal mentoring program at a software laboratory in Beijing, China, provide insight into the core mechanisms by which such programs produce network change: access to organizational elites, participation in semiformal foci, enhanced social skills, and legitimacy-enhancing signals. These mechanisms are theorized to lead to an expansion in protégés' networks, relative to those of non-participants in formal mentoring. Legitimacy-enhancing signals are theorized to enable female protégés to derive greater network benefit from formal mentoring than their male counterparts. Empirical support for these propositions comes from a longitudinal quasi-experiment involving 75 employees who experienced the treatment of formal mentoring and 64 employees in a matched control group. A second empirical strategy, which exploits exogenous variation in the timing of treatment and enables a comparison of the post-program networks of one treated group to the pre-program networks of another treated group, provides corroborating support. These findings contribute to research on the efficacy of formal mentoring, gender and workplace networks, and the cumulative advantage or disadvantage that can arise from network change.

Introduction

There is by now a wealth of evidence linking the nature and quality of interpersonal networks within organizations to various indicators of individual attainment—for example, performance evaluations and rewards (Burt 1992), promotions (Podolny and Baron 1997), relative power and influence (Brass 1984),

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and career satisfaction (Seibert, Kramer, and Liden 2001). Yet, despite the importance of networks for individual success, remarkably little is known about what organizational practices, if any, help employees build interpersonal connections and how these effects might vary by type of employee. The evidence that does exist on how networks change in response to new practices such as training (Burt and Ronchi 2007), job rotation (Campion, Cheraskin, and Stevens 1994), and mentorship (Dreher and Ash 1990) is based on research designs that do not support causal identification—for example, studies that lack a credible control group against which to compare the outcomes of the treatment group, draw inferences based on cross-sectional or correlational data, or inadequately separate selection from treatment effects (for a review, see Van de Valk and Constatas [2011]).

This article helps fill the void in our understanding of the efficacy of organizational practices designed to change workplace networks. It does this by examining a pervasive practice—formal mentoring—that is widely thought to alter protégés' networks in a manner that supports individual attainment (Hezlett and Gibson 2007). Formal mentoring is also believed to help women overcome deficiencies in network access—for example, to powerful actors or dominant coalitions in the organization—and is therefore proposed as an important means to addressing gender inequality in the workplace (Noe 1988). Yet, the conceptual arguments and empirical evidence on the effectiveness of formal mentoring, in general, and as a practice to ameliorate gender inequality, in particular, are still inconclusive (Allen et al. 2008; O'Brien et al. 2010).

In this article, I seek to make three main contributions. First, I draw on qualitative interviews with 40 past protégés in a formal mentoring program to surface the core mechanisms linking formal mentoring to network change. These mechanisms serve as the building blocks for theory development about the conditions under which formal mentoring can be expected to produce network expansion and gender differences in who stands to experience the greatest network benefit. The qualitative evidence also helped inform the design of a subsequent quasi-experiment conducted with a formal mentoring program targeted to high-potential employees in a software development laboratory in Beijing, China. Second, I report the results of this experiment, which involved 75 employees who experienced the treatment of formal mentoring and 64 employees in a matched control group. The research design overcame many of the limitations prior studies faced in identifying causal effects. It provided two pathways for causal identification: (1) a differences-in-differences analysis (Angrist and Pischke 2009) of changes between the pre- and post-program networks of treatment and control group members; and (2) a comparison of the pre- and post-program networks of two treatment group cohorts whose timing of program participation varied exogenously.

Finally, I elucidate and help resolve a conceptual puzzle about the differential effects of formal mentoring on the workplace networks of women and men. One set of arguments suggests that men will experience greater network expansion as protégés in formal mentoring than women will experience, while another predicts the opposite effect. I hypothesize that, through the mechanism of legitimacy-enhancing signals, such programs will provide greater network benefit to female participants than to their male counterparts. Results from the quasi-experiment were consistent with this expectation. These findings contribute to research on

formal mentoring, gender and workplace networks, and the cumulative advantage or disadvantage that can arise from network change.

The theoretical arguments and empirical analyses that follow are informed—and to some extent constrained—by the distinctive cultural and institutional features of work organizations in China. For example, work organizations in China often have power structures that are more starkly gendered than those found in organizations based in Western societies. Indeed, in the software development lab that served as the research site, there was a dearth of senior women who could potentially serve as formal mentors. It was therefore not possible to investigate the potential moderating role of gender match between mentors and protégés in network expansion. Similarly, the patriarchal nature of many Chinese work organizations requires even greater attention to the potential threat to causal inference arising from selection bias. For example, if women have to demonstrate greater competence than men to be designated as high potentials, then unobserved gender differences in ability could potentially confound estimates of the treatment effect of formal mentoring on the networks of male versus female protégés. This possibility underscores the importance of employing fixed-effect specifications of the kind described below to account for such unobserved heterogeneity.

Theory

Workplace Networks and Attainment

Research on social networks and individual attainment has considered the roles of network structure—for example, weak ties (Granovetter 1995) or structural holes (Burt 1992)—of social resources that can be accessed through ties (Lin 2001), and of the interplay between the two (Seibert, Kramer, and Liden 2001). The present investigation draws on the social resources perspective, which suggests that the size and nature of workplace contacts mobilized can influence the quality of resources that flow to employees and thereby influence their attainment. For example, in their longitudinal study of social networks and upward mobility, Podolny and Baron (1997, 687) reported that each additional (recently formed) task advice tie more than doubled an employee's odds of promotion in the following year. Each additional strategic intelligence tie had a comparable effect size.

Indeed, research on interpersonal networks and employee performance has consistently documented a positive association between network size, or degree centrality, and individual outcomes such as job satisfaction (Flap and Völker 2001), sales performance (Moran 2005), job performance ratings (Sparrowe et al. 2001), and income (Carroll and Teo 1996). While studies linking social resources obtained through networks to attainment have proliferated, very little is known about the efficacy of network intervention.

Network Intervention

Network intervention refers to “purposeful efforts to use social networks or social network data to generate influence, accelerate behavior change, improve

performance, and/or achieve desirable outcomes among individuals, communities, organizations, or populations” (for a review, see Valente [2012], 49). There are four broad kinds of network intervention: (1) identifying individuals based on a network property to exert influence on others (e.g., Valente and Pumpuang 2007); (2) targeting a change initiative to a subgroup within a network (e.g., Meltzer et al. 2010); (3) stimulating peer-to-peer interaction to create information cascades (e.g., Aral and Walker 2011); and (4) deliberately altering the network to change outcomes of interest (e.g., Thomas et al. 1998). My focus is on the fourth kind of network intervention—specifically, organizational practices such as formal mentoring that are designed to help employees build valuable social connections in the workplace.

Formal Mentoring and Workplace Networks

Formal mentoring programs have diffused broadly across organizations, including work organizations in China (Bozionelos and Wang 2006). Survey estimates indicate that one-third to two-thirds of workers have participated in a mentoring relationship (Seibert 1999). Mentors are defined as experienced and knowledgeable individuals who are committed to providing career and psychosocial support to one or more protégés (Kram 1985). Although many relationships in the workplace can include a developmental component, I focus on traditional forms of mentoring—hierarchical relationships focused on protégé development (Higgins and Kram 2001). Informal mentoring refers to relationships that develop spontaneously and often last a long time, whereas formal mentoring involves relationships that arise from organizational intervention—typically in the form of voluntary assignment or matching of mentors and protégés—and often exist for a shorter duration (Ragins and Cotton 1999).

Formal mentoring programs are often targeted to specific employee populations, such as new employees, senior managers, or high-potential employees. Indeed, a survey of 246 US corporations found that only 10 percent of firms with formal mentoring programs made them generally available to all employees; the remainder targeted them to specific populations (Douglas and McCauley 1999). The conceptual arguments below pertain to formal mentoring targeted to high-potential employees. Surveys indicate that nearly a third of the formal mentoring programs in place in US corporations are targeted to this population (Douglas and McCauley 1999).

Formal mentoring, whether targeted or not, is widely believed to have a positive influence on protégés’ career outcomes and subjective well-being—for example, promotions, income, organizational commitment, turnover intentions, job satisfaction, self-esteem, work stress, and work-family conflict (for a review, see Underhill [2006]). These distal outcomes are thought to arise in part through formal mentoring’s proximal effects on workplace networks (Hezlett and Gibson 2007). For example, introductions made by mentors on behalf of protégés in the course of formal mentoring lead to “an expanded social network that provides the protégé with other sources of contacts, advice, social support or strategic information” (Wanberg, Welsh, and Hezlett 2003, 94). In other words, network expansion is often a core objective of formal mentoring programs.

Yet, the empirical evidence on formal mentoring's overall effectiveness remains inconclusive—in part because the extant literature has not employed research designs that support causal identification—and the mechanisms by which mentoring changes networks remain unclear. Indeed, a recent meta-analysis of research methods used in more than 200 mentorship studies found that over 90 percent were based on cross-sectional data. Approximately 70 percent did not specify or failed to distinguish the form of mentoring studied—for example, formal versus informal—even though these distinctions are conceptually relevant. Only 5 percent used qualitative methods to uncover the mechanisms by which mentoring works or does not work (Allen et al. 2008).

To my knowledge, only three prior studies have employed designs that enable researchers to make causal claims—one employed a longitudinal quasi-experiment (Seibert 1999) and the other two random assignment of participants to control and treatment conditions (Eesley and Wang 2014; Egan and Song 2008). These studies examined outcomes such as employee attitudes, supervisor performance ratings, and rates of entrepreneurship but did not seek to measure changes in workplace networks. Thus, we have heretofore lacked credible causal evidence on whether formal mentoring affects protégés' workplace networks and, if so, whether it has differential consequences for women relative to men.

Gender Differences in Expected Effects of Formal Mentoring

A robust literature has examined gender differences in workplace networks. For example, Brass (1985) reported that, although men and women in a newspaper publishing company appeared to build networks equally well, they tended to form sex-segregated networks. As a result, women were less central in men's networks, especially those of the dominant coalition in the organization. Campbell (1988) also found differences in the networks of employed men and women, with the former having networks with greater occupational range and socioeconomic diversity than the latter. In a similar vein, Ibarra (1992) found in a study of an advertising firm that men tended to form homophilous networks for both instrumental and expressive purposes, whereas women exhibited a differentiated pattern of homophily in forming expressive ties and heterophily in building instrumental ties.

In a large financial services organization, women were less likely than men to have the resources and positions that would bring them into contact with and build relationships with high-status employees (McGuire 2000), and women received less informal help than men even when they had jobs in which they controlled resources and had network contacts who also controlled resources (McGuire 2002). More recently, Kleinbaum, Stuart, and Tushman (2013) analyzed e-mail data in a large technology firm and found that women had a greater number of contacts than men and communicated at an elevated rate with other women inside and outside their business units and offices. Men exhibited homophily only in within-office communication.

To the extent that formal mentoring changes workplace networks, existing theory leads to competing expectations about potential gender differences in these effects. On the one hand, men can be expected to derive greater benefits

from formal mentoring than women because they are better able to translate a given structural position into network advantage (McGuire 2002; Roth 2006). As protégés, men also receive more career development support, such as career counseling or introductions by mentors to powerful others (Ragins and Cotton 1999). Moreover, for men, taking action on this support—for example, following up on introductions made—is more consistent with their gender role than it is for women (Ragins 1999). These advantages for men over women are likely to be amplified in settings with sharply gendered power structures of the kind found in many Chinese work organizations.

On the other hand, women can be expected to accrue a disproportionate share of benefits from formal mentoring because they often enter these programs from structural positions that, relative to those held by men, provide inferior access to powerful organizational actors (Moore 1992). Prior to participation in formal mentoring, women are also less likely to be visible in the organization (Kalev 2009) and more likely to be marginalized as a result of exclusionary pressures (Mehra, Kilduff, and Brass 1998). For example, McGuire's (2000, 519) study of employees in a large financial services company concluded that: "Structural exclusion from high-ranking and resourceful positions, not a lack of networking knowledge or skills, prevented ... women... from forming ties to powerful network members." Insofar as formal mentoring serves to rectify these past inequities, it should provide greater benefit to women than to comparably skilled men. In short, the question of whether formal mentoring will differentially expand men's or women's networks has heretofore remained a conceptual puzzle.

Unpacking the Mechanisms by Which Formal Mentoring Produces Network Change

To gain conceptual clarity on this puzzle, I begin by unpacking the mechanisms by which formal mentoring produces network change and consider how each might operate differently for men versus women. The qualitative interviews I conducted with 40 past participants in a formal mentoring program (details provided below) revealed four core mechanisms that link formal mentoring to network change: (1) access to influential organizational actors; (2) involvement in project teams that serve as foci for new tie formation; (3) social skills acquired by the protégé; and (4) legitimacy that derives from the signal of a protégé's formal association with a respected senior person.

Formal mentoring can be expected to provide improved access to organizational elites, as mentors introduce protégés to their network contacts. These introductions will, in turn, enable protégés to expand their networks. For formal mentoring targeted to high-potential employees, however, there is no reason to expect that these introductions will provide differential benefits to one gender over the other. This is because, although women in general are often excluded from an organization's elite circles (Kanter 1977), high-potential women are likely to begin with comparable levels of network access as men. Indeed, in one empirical setting, high-potential women had closer ties and even broader network range than high-potential men (Ibarra 1997). Thus, high-potential men and women will experience a comparable change in network access from participating in formal mentoring.

Insofar as formal mentoring involves the assignment of a protégé to a work group or project team, these allocation choices provide another conduit to network expansion. Work groups and project teams can be thought of as elements of the semiformal organizational structure (Biancani, McFarland, and Dahlander 2014). They serve as foci for interaction and facilitate the formation of new ties (Feld 1981). So long as mentors are not biased in their propensity to assign male or female protégés to work groups or project teams, there is again no reason to expect that gender differences in network expansion will arise from this mechanism.

Social learning represents a third pathway from formal mentoring to network expansion. For example, in qualitative studies of formal mentoring's benefits and challenges, protégés routinely report that the experience sharpened their ability to understand the perspectives of colleagues in other organizational subunits, boosted their self-confidence in navigating complex interpersonal dynamics, and improved their ability to integrate different problem-solving techniques (Eby and Lockwood 2005). This learning arises from exposure to a broader range of organizational subunits and organizational actors. Assuming that male and female protégés receive comparable exposure during formal mentoring to new subunits and actors, they should also experience similar rates of network expansion stemming from enhanced social skills.

Whereas the first three mechanisms are not likely to have differing consequences for men's and women's networks, I theorize that the fourth mechanism will produce greater network expansion for women than for men. Consistent with Spence's (1973) theory of signaling, the assignment of a protégé to a respected senior mentor can convey the protégé's worth to others in the organization (Ramaswami et al. 2010). As Burt (1998, 24) explains, "Company leaders don't have time to check into the credibility of everyone making a bid for broader responsibilities. They are looking for fast, reliable cues about managers on whom they do not already have information." Burt (1998, 27) goes on to argue that direct supervisors make for poor sponsors because they are expected to endorse their subordinates, while more organizationally distant advocates such as formal mentors add a "corroborating external voice" that constitutes a more credible signal of worth.

In many organizational settings, women—even high-potential women—are likely to be viewed as less legitimate than equally competent men (Ridgeway 1997). Thus, the positive signal that comes with the assignment to a well-regarded mentor will provide a greater legitimacy benefit to women than to men. Enhanced legitimacy makes a person more attractive as a potential network partner for two reasons. First, people prefer affiliating with others whom they believe to be connected to organizational elites (Kilduff and Krackhardt 1994). Second, more legitimate actors are also more likely to accrue valuable social resources and tend to be sought after as exchange partners (Thye 2000). Assuming that protégés generally seek to forge new network ties, female protégés are therefore likely to experience greater network expansion than will male protégés. In the context of many Chinese work organizations, where women are especially likely to be marginalized, the benefits of legitimacy-enhancing signals are even more likely to accrue to women than to men.

Taken together, these arguments lead to a baseline expectation that formal mentoring will lead to network expansion for protégés, relative to comparable non-participants in formal mentoring. At the same time, the legitimacy-enhancing signals that arise when protégés are affiliated with well-regarded mentors suggest that gender will moderate this effect. In other words, whereas formal mentoring can be expected to provide network benefits to both men and women through increased access to organizational elites, potential participation in semiformal foci, and enhanced social skills, women will receive a “double benefit” (cf. Briscoe and Kellogg 2011) in the form of enhanced legitimacy. I therefore expect:

Hypothesis 1: People who participate in targeted formal mentoring will experience greater network expansion than comparable individuals who do not participate in targeted formal mentoring.

Hypothesis 2: Gender will moderate the effects of formal mentoring on network expansion, such that women will experience greater network expansion from targeted formal mentoring than will men.

Method

Empirical Setting and Program Description

I tested these hypotheses in a software development laboratory, which was located in Beijing, China, but was part of a US-based global technology products and services firm. The laboratory employed several thousand people and was organized into departments, corresponding to the firm’s global software brands and to various cross-brand programs. Although most employees were born and educated in China, they were generally proficient in English.

Over the years, the firm had shifted an increasing share of its software development activity from the United States to less expensive locations such as India and China. As a result, the software development laboratory in China was experiencing rapid growth. The scarcity of competent managerial talent represented an important constraint on the lab’s ability to grow. The head of the lab therefore decided to implement a targeted formal mentorship program, referred to internally as the “shadowing program,” which was targeted to well-performing employees who were thought to have management potential. Individuals were nominated for the program by their managers. A program manager in human resources made final selection decisions and then matched protégés to mentors based on expressed learning needs.

Matches were made across departmental lines—that is, selected individuals worked in a different department than the mentors to whom they were assigned—so program participants could gain breadth of exposure. Mentors were of comparable rank and thus had similar status in the organization. As noted above, it was not possible in this setting to test for differences in the treatment effect based on the gender match between mentor and protégé—a key variable that can influence mentorship outcomes (Ragins 1999).

The mechanics of the program worked as follows. Those selected for the program were assigned to “shadow” a more senior leader for a finite period, typically

the equivalent of 12 business days spread out over two to three months. The protégé and his or her mentor had an initial meeting to discuss objectives. The mentor would then grant the protégé access to his or her electronic calendar. Protégés could attend any meeting on the calendar, except for sensitive career discussions between the mentor and a direct report—for example, a performance review. In some cases, mentors would also assign protégés a discrete project to complete during the assignment. Although the list of protégés was not formally announced, people generally knew who was in the program and whom they were shadowing at any given point in time. Upon conclusion of the program, protégés returned to their original job roles.

Qualitative Analysis: Mechanisms Linking Formal Mentoring to Network Change

Insight into the mechanisms by which formal mentoring produced network change for protégés came from an analysis of 40 semi-structured interviews conducted with past program participants. The interview protocol is provided in the appendix. Of the 31 program alumni who were invited to participate in the interviews, 22 agreed to do so. In addition, all 11 mentors who had taken on a protégé in the past and all seven program administrators from human resources agreed to participate in the interviews. Thus, the overall response rate was 82 percent. Interviews with mentors lasted 30 minutes, while those with protégés and program administrators lasted 45 to 60 minutes. Interviews were tape-recorded and transcribed. Because most interviewees were not native English speakers, I edited some of the quotations reported below for grammar and syntax.

I analyzed the qualitative data using a software tool—Atlas.ti. I coded all mentions of factors that respondents felt contributed to network change following program participation. I started by developing detailed codes for each of these mentions, such as “Status Enhancement,” “Sponsorship,” “Project Teams,” and “Increased Confidence.” Some mentions were assigned to multiple codes. Later, I grouped these specific codes into four code families: “Access to Influential Organizational Actors,” “Participation in Semiformal Foci” (e.g., work groups and project teams), “Enhanced Social Skills,” and “Legitimacy-Enhancing Signals.” Table 1 provides representative quotations associated with each of these code families and shows the number of times they were mentioned by interviewees.

A Network Intervention Quasi-Experiment

After completing the qualitative interviews, I worked with the company to design a quasi-experiment of the program’s effects on workplace networks. The company identified 102 people to participate in an upcoming iteration of the program. Consistent with the practice from prior years, these individuals were selected for the program through a two-stage process: (1) supervisors nominated subordinates whom they believed to have significant potential for advancement within the organization; and (2) the human resource professionals who managed the program chose a subset of nominated employees based on the program’s available capacity in that year, employees’ past performance ratings, and the

Table 1. Qualitative Evidence—Representative Quotations

Mechanism	Representative examples	Mentions
Access to influential organizational actors	<p>“Before the experience, I knew only a few people in [the mentor’s] group. Afterward, I built very good relationships with his entire team—not only here but also in the US.” –Male, Protégé</p> <p>“I would say that I added half a dozen people to my network, mostly direct reports of [my mentor] or people two levels down. Now I know their mission and what kind of resources they have. That has given me ideas about resource borrowing and rotation between my team and his.” –Male, Protégé</p> <p>“One reason the shadowing program works is that it gives you visibility in another part of the organization.” –Female, Protégé</p> <p>“As a shadow, I was able to attend meetings of the [lab head’s] direct reports. Before the meeting and during breaks, I got to know those people through informal chats.” –Female, Protégé</p>	32
Participation in semiformal foci (e.g., work groups and project teams)	<p>“Because of the shadowing experience, [shadows] extend their networks further—not only to me but to all of my subordinates. If shadows are involved in project work during the assignment, they will often call on my organization in the future when the need arises.” –Male, Mentor</p> <p>“During the shadowing program, [my mentor] had me participate in many projects so I could learn about what other people in his group were working on. That helped me expand my social network.” –Male, Protégé</p> <p>“[My mentor] gave me an assignment—to coordinate between our lab and the US organization to put together an event for a delegation coming to China. I identified the key players on both sides and worked with them to pull it together.” –Female, Protégé</p> <p>“[One of my prior shadows] helped me prepare speeches and presentations during her assignment. As part of that work, she had direct working interaction with my direct reports and other technical people in my group.” –Female, Mentor</p>	16
Enhanced social skills	<p>“It will be more comfortable for me now to call [my mentor] and people in [my mentor’s department]. If I have a request, I have a better chance of getting help from them now.” –Male, Protégé</p>	11

(Continued)

Table 1. *continued*

Mechanism	Representative examples	Mentions
	<p>"In this culture, the hierarchy of the organization is significant. The shadowing program increases their comfort level with senior people. It brings upper levels within the reach of shadows. If you put people in a position where it is okay to ask questions, it changes things." –Male, Mentor</p> <p>"Before I understood the value of networking theoretically, but the shadowing experience gave me a chance to practice it. I became more confident. It proved to me that I can be helpful to others. Now I feel I can reach out to people even if I don't know them."</p> <p>–Female, Protégé</p> <p>"The shadowing program helped me get to know people from the other group so I can ask for help when I need it ... It makes collaboration easier." –Female, Protégé</p>	
Legitimacy-enhancing signals	<p>"Being a shadow says that your manager cares about your career. He wants to increase your exposure. It's a good sign. You're considered a high potential person—a technical resource for the future. It makes you desirable for others to get to know." –Male, Protégé</p> <p>"At every meeting, [my mentor] would introduce me and tell people I was his shadow. I think the introduction helped send a signal about me. When I followed up with people, I got responses very quickly because it was known I was working with [the mentor]. Even when he introduced me by e-mail, they'd respond quickly." –Male, Protégé</p> <p>"I felt I got some extra respect from being a shadow. It meant that the company recognized me and wanted to develop me." –Female, Protégé</p> <p>"The shadowing experience boosted my reputation with [my mentor's] direct reports and his broader network."</p> <p>–Female, Protégé</p>	32

strength of support they received from supervisors and other senior leaders who had knowledge of their work.

Once program participants were notified of their selection to the program, the head of the software lab sent them an e-mail informing them of the study and inviting them to participate. They were told that the study's objective was to help assess the effectiveness of the program and to identify ways to improve its design. I then sent them a follow-up e-mail explaining that participating in the study entailed completing surveys before, during, and after the program. Of the 102 people invited to participate, 91 agreed to do so by completing the pre-program survey (89 percent participation rate). Based on their responses to the pre-program survey, which included a section on work history, I identified 16

individuals who had previously participated in a shadowing or other comparable formal mentoring program. For example, the company sometimes assigned high-potential employees to serve as an executive assistant to a senior leader and ran other smaller-scale mentorship programs. I excluded those individuals from the estimation of the treatment effect because their pre-program networks likely included the past effects of participation in such programs. The results reported below were substantively unchanged—though somewhat attenuated—when these 16 individuals were included in the analysis. The resulting treatment group consisted of 75 people.

The company did not agree to random assignment of eligible participants into treatment and control groups because they worried about unintended signals such a procedure might send to their most valued employees. Instead, I worked with the company's human resources (HR) department to construct a matched control group. Because the firm's internal policies prohibited the sharing of employee records with external researchers, it was not possible to employ standard techniques such as propensity score matching (Rubin 2006). Instead, the firm agreed to implement the following matching procedure: for each program participant, a human resources representative identified two people who: (1) were at the same salary band; (2) had the same performance rating in the prior year; (3) had the same tenure within the organization; (4) worked in the same office; and (5) had not previously participated in the formal mentoring program.

When, as in most cases, more than two people met these criteria, the HR representative randomly selected two from the eligible list. In some cases, there was only one person who matched these criteria. In total, 189 people were identified through this procedure and invited to participate in a research study on the topic of workplace social networks. No mention was made of the formal mentoring program in the communication to these individuals. They were simply told that they would need to complete three network surveys to participate in the study. Of the matched control group, 85 agreed to participate by completing the first survey (45 percent participation rate). Based on their responses to the first survey, which included questions about their work history, I identified 21 who had previously participated in some form of targeted formal mentorship or shadowing program. For comparability with the treatment group, these individuals were excluded from the analysis, resulting in 64 matched control group employees.

Table 2 provides evidence that the matching procedure was effective: there were no statistically significant differences between the treatment and matched control groups on observable characteristics, including the number of contacts they reported mobilizing in the two months prior to the start of the formal mentoring program. At the same time, it is important to note that the treatment and matched control groups may have varied on unobserved characteristics. The former were, for example, selected partly on the basis of their (unobserved) advancement potential, while the latter were not selected on this basis. Because the treatment and matched control groups may have differed in unobserved ways, it was necessary to implement an alternative identification strategy (described below) based on a comparison of two subsets of treatment group individuals.

Table 2. Characteristics of Treatment Group and Matched Control Group—Comparison of Means

Variable	Female employees			Male employees			All employees		
	Matched control group	Treatment group	<i>t</i> -statistic/ <i>p</i> -value	Matched control group	Treatment group	<i>t</i> -statistic/ <i>p</i> -value	Matched control group	Treatment group	<i>t</i> -statistic/ <i>p</i> -value
Age (years)	38.6	37.9	0.311/0.758	35.7	35.1	0.786/0.434	36.1	35.7	0.512/0.610
Tenure within firm (years)	12.5	12.5	0.022/0.983	9.85	9.83	0.014/0.989	10.3	10.6	-0.274/0.785
Proportion holding advanced degree	0.727	0.700	0.155/0.878	0.793	0.764	0.357/0.722	0.781	0.747	0.475/0.636
Number of prior promotions	1.91	1.50	1.024/0.315	1.34	1.27	0.417/0.678	1.44	1.33	0.717/0.475
Contacts reported in pre-program survey	8.18	7.10	0.476/0.638	10.94	8.78	1.500/0.137	10.47	8.33	1.741/0.084
Proportion female	-	-	-	-	-	-	0.172	0.267	-1.337/0.183

Note: *N* = 64 for matched control group, of which 53 were men. *N* = 75 for the treatment group, of which 55 were men.

Both groups completed online network surveys before and two months after their formal mentoring assignments. The pre-program survey included questions about respondents' work histories and sociodemographic characteristics. Each survey included four network name generators, which were adapted from previous studies of workplace social networks (Podolny and Baron 1997): (1) task advice; (2) mentorship; (3) strategic intelligence; and (4) friendship. Because employees could potentially have obtained social resources from several thousand potential colleagues, it was not possible to use the roster method to identify contacts. Instead, participants listed initials of network contacts through free recall.

Sample Attrition

Sample attrition is a well-recognized problem in longitudinal network studies (Huisman and Steglich 2008). Of the 139 people who completed the pre-program survey, 73 did not complete the post-program survey (attrition rate of 52 percent). Those who did not complete the post-program survey were statistically indistinguishable from those who did on all observable characteristics, including reported network size prior to treatment. Nevertheless, to account for potential bias stemming from sample attrition, I conducted a robustness check (described below) using inverse probability treatment weights (Robins, Rotnitzky, and Zhao 1995).

Two Empirical Strategies for Causal Identification

Because the quasi-experiment did not entail random assignment of participants to treatment and control groups, I employed two complementary empirical strategies to recover causal effects. The first identification strategy included both the treatment and matched control groups. I used differences-in-differences estimation. The differences-in-differences estimator represents the difference between the pre-post, within-subjects differences of the treatment and control groups (Angrist and Pischke 2009). In some specifications, I included individual fixed effects to account for all time-invariant unobserved heterogeneity among participants and non-participants. Fixed effects were especially important to include given potential unobserved differences in the abilities of women and men selected for the program.

The second identification strategy helped address threats to the validity of the first strategy. The matched control group was not involved in this analysis, thereby obviating the concern that this group might have differed from the treatment group on unobserved characteristics (e.g., advancement potential or susceptibility to survey fatigue). Instead, this strategy involved comparing two subsets of treatment group employees. In particular, it took advantage of a unique feature of the way the program was implemented.

Because the available capacity of mentors was limited, the program was implemented in two separate cycles. Interviews with program administrators and participants, as well as my own observations, confirmed that assignment of participants to cycles was based on factors that were exogenous to individual ability or perceived managerial potential—for example, departments facing an impending deadline might prefer to send people nominated from the department to the later program cycle, while departments that had just completed an important project milestone or recently hired new staff might prefer to send their nominated people to the first cycle. In other cases, individual-level factors that were unrelated to ability or perceived potential—for example, previously scheduled business trips or training programs—determined the choice of cycle.

Table 3 compares the observed characteristics of these two groups. Consistent with the notion that exogenous factors led to the assignment of people to program cycles, none of the differences—including the number of contacts mobilized in the two months before the start of the mentoring program—was statistically significant.

To estimate the causal effect, I compared the post-program reported networks of participants from one cycle to the pre-program reported networks of participants from the other cycle. One cycle of the treatment group therefore served as the “control” against which the outcomes of the other cycle were compared. The key advantage of this approach over having a matched (but not randomly assigned) control group is that it better accounts for potential selection bias and unobserved heterogeneity. Because both groups of participants were selected for (and ultimately received) the treatment of program participation, they were likely to be comparable on observed *and* unobserved factors.

In principle, one could consider either cycle the treatment group and the other the control group. However, if the first cycle were considered the treatment group

Table 3. Characteristics of Cycle 1 and Cycle 2 Treatment Groups—Comparison of Means

Variable	Cycle 1	Cycle 2	<i>t</i> -statistic/ <i>p</i> -value
Age (years)	35.7	35.7	−0.017/0.962
Tenure (years)	10.6	10.5	−0.077/0.939
Proportion holding advanced degree	0.727	0.774	0.454/0.651
Proportion female	0.204	0.354	1.450/0.151
Number of prior promotions	1.21	1.50	1.313/0.194
Number of contacts reported in initial (pre-program) survey—All	8.07	8.71	0.429/0.669
Number of contacts reported in initial (pre-program) survey—Females	6.44	7.64	0.407/0.689
Number of contacts reported in initial (pre-program) survey—Males	8.49	9.30	0.455/0.651

Note: *N* = 44 for Cycle 1; 31 for Cycle 2.

and the second the control, there would be risk of cross-contamination of the control group. For example, participants from the first cycle might talk about their experiences in the program with those in the second cycle. This communication could then influence the network actions of second-cycle participants and thereby distort their reported networks prior to treatment. To address this possibility, I compared the post-program reported networks of participants from the second cycle to the pre-program reported networks of participants from the first cycle.¹

Measures and Estimation

In constructing the dependent variable, I drew on prior research linking workplace networks to various indicators of individual attainment. For example, Podolny and Baron (1997) found that task advice and strategic intelligence ties were associated with increased likelihood of upward job mobility. They reported mixed results for the relationship between mentorship ties and promotion chances: such ties were beneficial only when mentors had “fate control.” By contrast, Seibert, Kramer, and Liden (2001) found strong support for the notion that larger mentorship networks are associated with greater career success. In the present empirical setting, mentors had “fate control” over protégés in that they often provided feedback to protégés’ line managers and offered input on later promotion decisions. Thus, all three kinds of network ties—task advice, strategic intelligence, and mentorship—could be expected to yield instrumental career benefits for employees in this setting.

Prior work indicates that Chinese workers often do not draw sharp distinctions between instrumental and expressive ties (Bian and Ang 1997; Bozionelos and Wang 2006), suggesting that friendship ties should also be considered alongside the other three kinds of ties. Thus, the dependent variable was a sum of the number of task advice, strategic intelligence, mentorship, and friendship ties a respondent mobilized in a given period. Comparable results to those reported

below were obtained when the dependent variable was based on just instrumental ties—that is, excluding friendship ties—and when it was weighted by tie strength.²

For the differences-in-differences estimation, I used an indicator variable, *Treatment*, which was set to 1 for program participants, and *Post-Program* was set to 1 for the period following the program. The interaction term, *Post-Program* \times *Treatment*, thus represents the overall treatment effect. To identify gender differences in the treatment effect, I used an indicator, *Female*, and the interaction terms *Post-Program* \times *Female* and *Post-Program* \times *Treatment* \times *Female*. (Note that the other variables that would typically be included to test for interaction effects—*Female*, *Treatment*, and *Treatment* \times *Female*—are time invariant and therefore subsumed by the fixed effects.)

For differences-in-differences estimation, I report results of conditional fixed-effect Poisson quasi-maximum likelihood regression models (Wooldridge 1997). This estimator is consistent under relatively weak assumptions: only the conditional mean needs to be correctly specified, and the standard errors account for potential over- or under-dispersion.

Results

Table 4 reports the results of the differences-in-differences estimation. Model 1, which represented the baseline, did not include employee fixed effects but instead included a number of control variables. None of the controls was statistically significant. In model 2, *Post-Program* is significant and negative, reflecting the decline in reported contacts among matched control group respondents. By contrast, *Post-Program* \times *Treatment* is positive and significant, suggesting an overall treatment effect consistent with Hypothesis 1. In model 3, *Post-Program* \times *Treatment* is not significant, while the three-way interaction, *Post-Program* \times *Treatment* \times *Female*, is positive and significant. That is, consistent with Hypothesis 2, the program led to an expansion of female participants' networks but not those of male participants. In models 4 and 5, I introduce employee fixed effects to account for time-invariant unobserved heterogeneity. Model 4 replicates the findings from model 2, also lending support to Hypothesis 1. Similarly, model 5's results largely mirror those from model 3 and provide support for Hypothesis 2.

Although the differences-in-differences estimates support Hypothesis 1 and Hypothesis 2, the descriptive statistics raise some concerns. Table 5 shows how the reported networks of treatment group and matched control group employees changed between the pre-program to the post-program survey. The treatment group reported an increase in the number of contacts mobilized, and this increase was statistically significant for female respondents. By contrast, the matched control group reported a decrease in the number of contacts mobilized, and this decrease was significant for the group as a whole and for male respondents.

It is unclear why the matched control group would report fewer contacts in the post-program survey than in the pre-program survey. To my knowledge, there were no changes in their job roles or responsibilities during this period. One possibility is that the control group suffered a relative decline in status because they were considered but ultimately not selected for the formal mentoring program

Table 4. Differences-in-Differences Estimates of Contacts Mobilized Poisson Quasi-Maximum Likelihood (PQML) Regression Coefficients

	Model 1	Model 2	Model 3	Model 4	Model 5
Age	-0.005 (0.038)	-0.004 (0.039)	-0.005 (0.039)		
Tenure	0.017 (0.035)	0.017 (0.035)	0.017 (0.035)		
Female	0.127 (0.187)	0.118 (0.192)	0.238 (0.398)		
Advanced degree (1 = yes; 0 = no)	0.083 (0.181)	0.085 (0.181)	0.082 (0.182)		
Number of prior promotions	0.041 (0.079)	0.042 (0.080)	0.047 (0.083)		
Post-program		-0.317*** (0.082)	-0.269** (0.086)	-0.308** (0.116)	-0.260* (0.124)
Treatment		-0.149 (0.160)	-0.060 (0.174)		
Post-program × Treatment		0.384*** (0.106)	0.172 (0.119)	0.406** (0.154)	0.199 (.0172)
Post-program × Female			-0.497† (0.284)		-0.506** (0.183)
Treatment × Female			-0.385 (0.469)		
Post-program × Treatment × Female			1.027** (0.321)		1.018*** (0.269)
Constant	2.127* (1.011)	2.217* (1.048)	2.228* (1.060)		
Employee fixed effects	No	No	No	Yes	Yes
Log likelihood	-486	-477	-469	-199	-191
Chi ²	2.45	18.40	34.08	8.02	45.41
Prob > Chi ²	0.785	0.018	0.000	0.018	0.000
N	158	158	158	158	158

Note: Robust standard errors. Models 4 and 5 include employee fixed effects, which subsume the (time-invariant) main effects of *Female*, *Treatment*, and *Female x Treatment*.

† $p < .10$ * $p < .05$ ** $p < .01$ *** $p < .001$ (two-tailed tests)

and therefore became less attractive as network partners for their colleagues. Alternatively, not being selected for the program might have made them less motivated and caused them to expend less effort in mobilizing social resources or responding to name generator questions in repeated surveys. Unfortunately, it is impossible to know which of these explanations might have accounted for the observed changes in the networks of matched control group employees.

These lingering questions about the comparability of the treatment group and matched control group underscore the importance of considering the second

Table 5. Descriptive Statistics: Workplace Networks of Treatment and Matched Control Groups—Comparison of Means

	Treatment group			Matched control group		
	All respondents	Female respondents	Male respondents	All respondents	Female respondents	Male respondents
Pre-program survey	8.33	7.10	8.78	10.47	8.18	10.94
Post-program survey	10.27	13.54	9.16	7.67	6.67	7.73
Difference: Post-program minus pre-program	1.94 (1.239)	6.44* (2.625)	0.376 (1.375)	-2.80* (1.405)	-1.52 (3.226)	-3.21* (1.547)

* $p < 0.05$; standard errors in parentheses.

identification strategy, which took advantage of exogenous variation in *when* treatment group employees participated in the program. Table 6 reports these results. Cycle 2 participants reported mobilizing 11.81 contacts in the post-program survey (two months after treatment), while Cycle 1 participants reported mobilizing 8.07 contacts in the pre-program survey (prior to treatment). This difference of 3.74 contacts was significant ($p < .05$). Female Cycle 2 participants reported mobilizing 17.0 contacts in the post-program survey, while female Cycle 1 participants reported mobilizing 6.44 contacts. This difference of 10.55 contacts was also significant ($p < .01$). Male Cycle 2 participants reported mobilizing 10.19 contacts, while male Cycle 1 participants reported mobilizing 8.49 contacts. This difference of 1.70 contacts was not significant. Overall, these results corroborate the differences-in-differences estimates and provide support for both Hypothesis 1 and Hypothesis 2. They suggest that the program had a positive treatment effect on workplace networks but that its benefits accrued primarily to female participants.

Although the changes in reported contacts mobilized might seem modest, and longer-term career outcomes were not measured in this study, prior research suggests that network changes of this magnitude can have dramatic career consequences. Recall that Podolny and Baron (1997) found that each additional (recently formed) task advice tie and each additional strategic intelligence tie more than doubled an employee's odds of promotion, and network size, or degree centrality, has been linked to a wide range of individual career outcomes. To the extent that these results generalize to other organizational settings, the changes in workplace networks detected in this experiment likely had meaningful implications for participants' careers.

Robustness Check and Extension

Although there were no significant differences in observable characteristics between those

Table 6. Second Identification Strategy—Mean Number of Contacts Mobilized by Cycle 1 and Cycle 2 Treatment Groups

	All respondents	Female respondents	Male respondents
Cycle 1, pre-treatment	8.07	6.44	8.49
Cycle 2, post-treatment	11.81	17.00	10.19
Difference: Cycle 2, post-treatment Minus Cycle 1, pre-treatment	3.74* (1.65)	10.55** (3.29)	1.70 (1.83)

Note: *N* = 65, of which 51 are male and 14 are female.
p* < 0.05 *p* < 0.01; standard errors in parentheses.

who completed both the pre- and post-program surveys and those who did not, I estimated the differences-in-differences models using inverse probability treatment weighting (IPTW) to better account for potential bias from sample attrition (Horvitz and Thompson 1952; Robins, Rotnitzky, and Zhao 1995). Specifically, I first estimated a logit model in which an indicator variable set to 1 for subjects who completed both surveys was regressed on age, tenure within the firm, and whether or not the person held an advanced degree. Next I re-estimated table 4, model 4, and table 4, model 5, while weighting the observations by the inverse of predicted probabilities. The results (not reported) were materially unchanged.

I also conducted a supplemental analysis to establish the role of legitimacy-enhancing signals as the primary mechanism producing greater network expansion for women than for men. I separately estimated for men and for women a fixed-effects regression with the three-way interaction term, *Post-Program* × *Treatment* × *Tenure*, and all relevant (i.e., time-varying) main effects and two-way interaction terms. Results (not reported) indicate that, for women, this three-way interaction term was significant and negative. In other words, although women who participated in the program experienced network expansion, their tendency to do so declined as their tenure in the organization increased. By contrast, the three-way interaction term was not significant for men. Thus, as women gained legitimacy through their own contributions during their time in the organization, they appeared to benefit less from the signal of being affiliated with a respected senior person. By contrast, perhaps because men did not start with a legitimacy deficit, the benefits of the program did not vary with their tenure in the organization.

Discussion and Conclusion

The goal of this study has been to examine how the introduction of a formal mentoring program changes workplace networks. Qualitative interviews with past program participants surfaced four core mechanisms that link formal mentoring to network expansion: access to influential organizational actors, participation in semiformal foci, enhanced social skills of protégés, and legitimacy-enhancing signals that arise from the affiliation with a prominent mentor. I theorized that, whereas the first three mechanisms can be expected to produce network expansion for both high-potential men and high-potential

women, legitimacy-enhancing signals will tend to expand women's networks more than men's. These propositions were tested in the context of a quasi-experiment conducted with a formal mentoring program targeted to high-potential employees in a software development laboratory in Beijing, China.

Differences-in-differences estimation with a treatment group and a matched control group supported the propositions, but the lack of random assignment of people to the two groups left lingering questions about their comparability. Exogenous variation in the timing of treatment enabled a second identification strategy that helped address these concerns: a comparison of the post-treatment networks of one treatment subgroup to the pre-treatment networks of another. This approach accounted for unobserved heterogeneity between the treatment and control groups and selection bias, which have plagued virtually all prior empirical investigations of the effects of formal mentoring (Allen et al. 2008; Underhill 2006; Wanberg, Welsh, and Hezlett 2003). This supplemental analysis corroborated the differences-in-differences estimates, suggesting that the program had a positive treatment effect on workplace networks but that its benefits accrued primarily to women.

Limitations and Directions for Future Research

Because these findings are based on data from a formal mentoring program targeted to high-potential employees and set in China, one must consider the extent to which these findings can be generalized to other employee populations and institutional settings. It is theoretically ambiguous how these effects can be expected to vary in formal mentoring programs that are not targeted to high-potential employees. On the one hand, women who are not considered high potential are more likely to occupy disadvantaged structural positions in the organization. So, they might benefit more than the women in this study from changes in opportunity structure. On the other hand, mentors might be less motivated to provide career development support to women who are not high-potential employees, such that they would benefit less than the women in this study. The net effect remains to be identified through future research.

Another question about generalizability arises from possible differences in the unobserved criteria used to select men and women into the formal mentoring program. Although there were no differences in the explicit criteria, it is possible that the women selected to the program had different underlying qualifications than their male counterparts. For example, they may have needed exceptionally strong abilities to overcome the implicit bias and discrimination that would otherwise have kept them from being noticed as high potentials. The fixed-effects specification used in the first empirical strategy helped account for this alternative explanation for why women may have benefited more from the program than men did.

The fact that the study was set in a particular location within China (Beijing) also raises questions about generalizability because of the country's changing institutional context (Zhao and Zhou 2004), the distinctive ways in which people think about and mobilize social resources in China (Bian 1997; Morris, Podolny, and Sullivan 2008; Ruan et al. 1997), and the heterogeneity of institutional

features and the role of social networks across regions within China (Bian 2002). In this particular empirical setting, national cultural differences were mitigated because the software development lab was part of a US-based multinational. For example, whereas Xiao and Tsui (2007) found that structural holes were detrimental to employees' career outcomes in the national cultural context of China, Merluzzi (2012) reported that—as in Western samples—there were positive returns to structural holes among senior managers working for a US-based multinational in China. Given that recent meta-analyses indicate that only one other study has examined formal mentoring in China (Allen et al. 2008), further work is clearly needed to understand the role of national cultural context in formal mentoring outcomes.

Finally, it was not possible in this particular field experiment to collect other network structural measures—such as density (Sparrowe et al. 2001), range (Reagans and McEvily 2003), or constraint (Burt 1992)—or examine the longer-term consequences of formal mentoring. Future research could profitably examine the effects of formal mentoring on not only the size and content of workplace networks but also their structure. Similarly, studies with longer time horizons could better examine whether formal mentoring's effects on networks are ephemeral or enduring and may allow for direct measurement of the theorized mechanisms that produce network change (e.g., legitimacy-enhancing signals).

Contributions

These issues notwithstanding, the study makes a number of noteworthy contributions. It is to my knowledge the first to provide quasi-experimental evidence of one of the main theorized benefits of formal mentoring: network expansion (e.g., Wanberg, Welsh, and Hezlett 2003). In particular, the use of two distinct identification strategies helps address concerns about selection bias that have plagued all prior attempts to estimate the network benefits of formal mentoring.

Next, it informs our understanding of the role of formal mentoring as a means to address gender inequality in the workplace (e.g., Noe 1988). Although firms that have introduced programs designed to increase the social connectedness of women have experienced modest reductions in gender inequality (Kalev 2009; Kalev, Kelly, and Dobbin 2006; Srivastava and Sherman in press), the core mechanism of network change was not directly observed or measured in this line of research. The present study provides direct evidence of this missing link. The result—that formal mentoring provided greater network benefit to women than to men—provides empirical support for McGuire's (2000, 519) contention that such programs are essential if companies seek to “equalize access to informal networks at work.”

Finally, by considering the consequences of targeted formal mentoring for employees who were otherwise comparable to participants but not selected to participate, the study provides suggestive evidence that can inform long-standing debates about networks and cumulative advantage (DiMaggio and Garip 2012). Participants in formal mentoring reported an expansion in valuable workplace networks, while matched non-participants reported a contraction. A number of factors—for example, survey fatigue or a decline in status from being considered but not selected for the program—could have accounted for the decline in control

group members' reported networks. Insofar as this decline was not just an artifact of survey fatigue but instead presented an actual loss of social capital, the program's network effects may have been a vehicle for promoting inequality. The benefits to the highest-potential employees may have come at the expense of those who were slightly less well regarded. In considering the effects of network intervention on the workplace as a whole, these findings underscore the need to consider not only the employee populations directly affected by the introduction of a new program or policy, but also the consequences for those who might be indirectly affected.

Conclusion

This study demonstrates the value of longitudinal field experiments in uncovering the causal effects of workplace practices such as formal mentoring on workplace networks. Such an approach promises to help network research in making the shift from simply characterizing internal network patterns and associated outcomes to producing tangible prescriptions about organizational practices that can reshape workplace networks in ways that support individual attainment and ameliorate inequality.

Notes

1. In supplemental analyses (not reported), I also compared the post-program networks of participants from the first cycle to the pre-program networks of participants from the second cycle. The former were somewhat larger than the latter, including for female participants; however, these differences were not statistically significant. This comparison was, however, more susceptible to the threat of cross-contamination than the one reported above.
2. I conducted a supplemental analysis to assess how the program's effects might have varied across different kinds of ties. Results (not reported) indicate that the program led to an expansion in task advice and mentorship ties, especially for female participants, but did not affect strategic intelligence or friendship ties.

Appendix: Interview Schedule

1. Could you please give a brief summary of your career history?
2. Why did you choose to participate in the shadowing program? What were you hoping to get out of the experience?
3. How was your participation in the program viewed by others?
4. Who were you matched to in the program? How was the match made? How much influence did you and the other person have in the decision?
5. Which unit [within the software lab] were you in at the time? Which unit was [your mentor/protégé] in?
6. Did you or your [mentor/protégé] have any specific objectives for the shadowing experience? If so, what were they?
7. How did you and [your mentor/protégé] first make contact with one another? What did you discuss? How did your interactions change over time?

8. How many hours per week did [your mentor/protégé] and you spend together? How did this vary over the course of the program? What was a typical day like?
9. Did participating in the program affect the size or composition of your network in the organization? If so, how do you think these changes arose? Could you provide some examples/illustrations? If there was no significant change, why do you think that was the case?
10. [Did your mentor introduce you to any of his/her contacts?/Did you introduce your protégé to any of your contacts?] If so, to whom? Were they internal or external contacts? If internal, which unit did they work in? What was the context in which this introduction took place? Did any of these introductions lead to the formation of new relationships? If so, how? If not, why do you think that was the case?
11. Did [your protégé/you] form any relationships as an indirect result of the program? If so, could you provide some examples? How did these relationships come about?
12. Do you believe [your protégé/you] changed personally or professionally as a result of the experience? If so, how?
13. How well do you think the shadowing experience met your objectives? Your [mentor's/protégé's] objectives? The organization's objectives?
14. How did the shadowing experience conclude?
15. What level of contact have you maintained with [your mentor/protégé] since the assignment ended? How would you characterize the relationship today?
16. As you reflect on the shadowing experience as a whole, what do you think were the most helpful aspects? The least helpful aspects? What, if anything, would you change about the experience?

About the Author

Sameer B. Srivastava is an Assistant Professor at UC Berkeley's Haas School of Business. His research examines the dynamics of social networks within organizations and their consequences for individual attainment. He has work that is recently published or forthcoming on the effects of female managers on the gender wage gap (in *AJS*) and on social influence in the US Senate (in *ASR*). Sameer holds a PhD in Sociology and Organizational Behavior from Harvard University.

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Partnership Ties Shape Friendship Networks: A Dynamic Social Network Study

Christoph Stadtfeld, *ETH Zürich, Switzerland; University of Groningen, The Netherlands*
Alex (Sandy) Pentland, *MIT Media Lab, Cambridge, MA, USA*

Partnership ties shape friendship networks through different social forces. First, partnership ties drive clustering in friendship networks: individuals who are in a partnership tend to have common friends and befriend other couples. Second, partnership ties influence the level of homophily in these emerging friendship clusters. Partners tend to be similar in a number of attributes (homogamy). If one partner selects friends based on preferences for homophily, then the other partner may befriend the same person regardless of whether they also have homophilic preferences. Thus, two homophilic ties emerge based on a single partner's preferences. This amplification of homophily can be observed in many attributes (e.g., ethnicity, religion, age). Gender homophily, however, may be de-amplified, as the gender of partners differs in heterosexual partnerships. In our study, we follow dynamic friendship formation among 126 individuals and their cohabiting partners in a university-related graduate housing community over a period of nine months ($N = 2,250$ self-reported friendship relations). We find that partnership ties strongly shape the dynamic process of friendship formation. They are a main driver of local network clustering and explain a striking amount of homophily.

Introduction

Partnership ties and friendship ties are the two most important types of choice-based¹ ties in personal social networks. Individuals profit from both types of relations in a number of dimensions. At the same time, the dynamics of partnership and friendship formation are tightly interlocked. In this article, we study how partnership ties shape friendship networks. In a dynamic social network study within a university-related housing community, we investigate how partners who move into a new social context seek agreement in their friendship choices, how they tend to befriend other couples, and how these mechanisms amplify the level of homophily in their friendship networks.

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Partnership ties and friendship ties have been widely studied. Both are known to be main sources of social support (Wellman and Wortley 1990; Dehle, Larsen, and Landers 2001; Agneessens, Waeye, and Lievens 2006), to have a positive influence on subjective well-being, such as life satisfaction and happiness (Burt 1987; Diener et al. 2000; Soons, Liefbroer, and Kalmijn 2009), and to be associated with mental and physical health (Berkman et al. 2000; Hughes and Waite 2009). In all these (partly overlapping) dimensions, individuals strongly benefit from having a partner² and from having friends. Burt (1987), Berkman et al. (2000), and Agneessens, Waeye, and Lievens (2006) argue, however, that the positive effect of a single partnership tie is stronger than that of a single friendship relation. Leaving aside the qualitatively different nature of partnership and friendship ties, one can argue that a partnership tie is more intimate and thus more valuable for an individual than a single friendship tie.

Partnership ties and friendship ties often connect individuals who are similar. Partner ties thus tend to be structurally homogamous. This means that partners are often more similar than expected from the variety in a particular marriage market. Homogamy is found in many attributes, such as ethnicity, religion, age, occupation, socio-economic status, and level of education (Kalmijn 1998; Blau 1977, 37). Friendship ties tend to be homophilic. This means that the emergence of ties between similar individuals is more likely than expected by the level of homogeneity in a certain population (Lazarsfeld and Merton 1954; Marsden 1988; McPherson, Smith-Lovin, and Cook 2001). Individual attributes for which homophily is commonly observed are those mentioned in the context of homogamy plus gender.

Despite a number of similar features, however, the partnership tie is unique. Partnership ties are generally monogamous in most cultures and therefore do not by themselves form large social networks at a given point in time. Nevertheless, partnership relations matter for the dynamics of friendship networks. We observe this, for example, as friends become partners, friends of partners become friends, couples befriend other couples, or friends of a partner are a matter of discussion in partnerships.

In this article, we do not study the emergence of partnership ties or their dissolution in the context of friendship networks. Rather, we investigate how pre-existing, stable partnership ties affect the formation, dissolution, and maintenance of friendship ties. We further discuss and explore how this interplay affects the shape of friendship networks. We argue that partnership ties are one of the main drivers of adult friendship formation and serve as nuclei in the emergence of complex structures in friendship networks. In particular, levels of clustering and homophily are strongly influenced by partnership ties.

In a longitudinal social network study, we investigate different clustering mechanisms and their interplay with homophily and homogamy in the formation of an adult friendship network among cohabiting couples. The individuals live in a university-related housing community with a high turnover of residents ($N = 2,250$ self-reported friendship ties between 126 individuals over 9 months who are connected by 62 pre-existing, non-changing partnership ties³). The data that we collected are unique in several ways. First, we are not aware of a comparable complete, longitudinal network data set consisting of adults *and* their part-

ners. Second, very detailed information on individual attributes is available that allows us to test homophily in five different attributes (age, gender, race/ethnicity, having children, religion) and its interplay with homogamy and clustering mechanisms around partnership ties. Finally, the data are collected in an emerging social context: the couples in our study come from different places around the world and rarely know each other beforehand. Of these couples, 44.6 percent moved in the same year our study began, and 87 percent did so within the past two years. Therefore, we observe a highly dynamic, evolving friendship network.

We find that although only approximately 13.3 percent of all positive relations in our study are partnership ties (86.7 percent are friendship ties), these partnership ties are nuclei that drive the dynamics of friendship formation. We find that friendship ties tend to emerge around partnership ties, embedding one or more partnership ties in dense local clusters. The fact that partnership ties tend to be homogamous amplifies the level of homophily in the friendship network. This amplification is found in the homogamous attributes of race/ethnicity, having children, and religion. In contrast, the strong tendency of individuals to choose friends of the same gender is almost hidden by the fact that through clustering around heterosexual partnership ties, many mixed-gender ties emerge that are not based on mixed-gender friendship preferences. In our study, religious homophily would be seriously overstated, whereas gender homophily might have been overlooked without controlling for clustering around partnership ties. These findings suggest that partnership ties are strong forces that shape the level of clustering and homophily in friendship networks.

Previous Studies on How Partnership Ties Shape Friendship Networks

The effect of stable partnership ties on the formation, dissolution, and maintenance of friendship relations has been theoretically discussed and empirically investigated in earlier studies. In the following, we present a brief overview.

Blau (1977) notes that “social associations depend on opportunity for social contact” (79, assumption 9) and “are more prevalent among persons in proximate ... social positions” (36, assumption 1). The partnership tie may therefore create opportunities for contact and communication with friends of the partner, which may eventually lead to the creation of new friendship ties with friends of the partner—a mechanism that is often described as triadic closure. A related outcome is discussed by the *dyadic withdrawal hypothesis* (Johnson and Leslie 1982; Milardo 1982; Kalmijn 2003), which states that the friendship networks of partners tend to shrink over time and to increasingly overlap. Milardo (1982) argues that these mechanisms are explained by the fact that both partners increasingly perceive themselves as a unit, as do friends of the partners. Investment in friendship relations are made as a unit. Johnson and Leslie (1982) state that individuals need to invest time in a partnership, which forces them to dissolve certain time-consuming friendship relations in which the partner is not involved. Another solution to lack of time may be to foster friendship relations

together with the partner, again creating opportunities for friendship formation between the partner and the friend. Gerstel and Sarkisian (2006) conclude that withdrawal from friendship networks may be related to the emotional and social demands of marriage, which may therefore be called a “greedy institution.” Parks, Stan, and Eggert (1983) link the emergence of transitive structures that incorporate a partnership tie with the psychological arguments of balance theory (Festinger 1957; Heider 1958). To become friends with the friends of the partner or to dissolve a friendship tie with someone with whom the partner is not friends may be strategies to reduce the stress associated with imbalanced friendship relations.

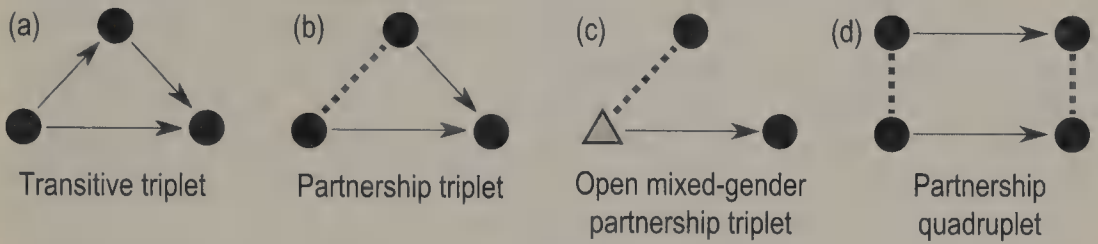
Kalmijn (2003) empirically investigates the predictions of the dyadic withdrawal hypothesis based on longitudinal, ego-centered social network data and finds evidence for partners’ increasing overlap and shrinking of friendship networks over the life course.

The cited studies theoretically argue that the change of personal friendship networks of two partners is expected to be interdependent, and specific social mechanisms (e.g., triadic closure, dyadic withdrawal) are described. In the following, we develop a related set of mechanisms about how friendship ties may change depending on the friendship ties of a partner. We take a dynamic social network perspective that builds on and goes beyond a personal network perspective (as taken in Kalmijn 2003). It accounts for the role of individuals and ties that are farther away from individuals, for example, the partner of friends and the partner of friends of the partner. At the same time, we propose hypotheses about how these social mechanisms differ from related mechanisms in friendship networks (e.g., triadic closure) and how they may influence the general shape (e.g., clustering, homophily) of an evolving friendship network.

Theory and Hypotheses

A typical pattern in the formation of friendship networks is the closure of transitive triplets: if person A considers B a friend and B considers C a friend, then there is a high probability that, eventually, A will consider C a friend as well. This recurring mechanism results in an empirical over-representation of transitive structures in social networks (Davis 1970; Newman and Park 2003). A *transitive triplet* is shown in figure 1(a). Granovetter (1973, 1362ff) argues that transitive closure may be driven by three processes.⁴ First, there is an increased likelihood of interaction between A and C because they have a common friend. This may eventually result in a friendship tie from A to C. This idea relates to focal closure (Kossinets and Watts 2009; Feld 1981), the increased likelihood of interaction between individuals who share a social context, and to the concept of propinquity (Festinger, Schachter, and Back 1974[1950]), the increased likelihood of interaction of those who are physically close. We may summarize these processes as *opportunity-related transitive closure*. Second, the two initial friendship ties may indicate a certain similarity of individuals that increases the chance of A and C also liking each other. Transitive closure would then be a byproduct of shared homophilic preferences on certain individual attributes (Moody 2001; Kossinets and Watts 2009; Wimmer and Lewis 2010) or of other shared preferences.

Figure 1. Four structures hypothesized to be relevant in the friendship formation process. Arrows indicate directed friendship ties (friendship nominations), dashed lines are partnership ties, and circles and triangles are individuals. A tie between a triangle and a circle is a mixed-gender tie. Ties between two circles can be either mixed- or same-gender ties.



We may summarize these mechanisms as *transitive closure due to shared preferences*. Third, transitive closure may be enforced by the three individuals involved, because only a closed triplet assures that the individuals' perception of the social situation is in *balance* (Heider 1958; Cartwright and Harary 1956; Newcomb 1961; Hummon and Doreian 2003).

The partnership tie allows the creation of a similar transitive structure: the *partnership triplet* emerges if individuals become friends with a friend of their partner or friends with the partner of a friend. The first case is shown in figure 1(b). The underlying social mechanisms are expected to be similar to the three closure mechanisms of transitive friendship triplets. First, the closure may be a matter of opportunity. Given that partners spend considerable time with each other, and share many social contexts and places, the likelihood of opportunity-related friendships between one partner and a friend of the other partner is high. In comparison to friendship triplets, opportunity-related closure can be expected to be even more prevalent because partners tend to share more time, social contexts and places, than average friends do. Second, partners tend to be similar in a number of dimensions, including their preferences. Transitive closure due to shared preferences is likely in the case of partnership triplets. In general, partners can be expected to have more similar preferences than average friends have, so again, the closure mechanism might be more prevalent than in the case of friendship triplets. Given the observation that homogamy is stronger than friendship homophily, triadic closure as a byproduct of a combined homophily/homogamy mechanism can be expected to be more prevalent where a partnership tie is part of the triplet. Third, the closure may be motivated by psychological balance arguments. Because the partnership tie is more intimate and more valuable on a number of dimensions compared with friendship ties, we can speculate that open partnership triplets are perceived as even more imbalanced than open friendship triplets. It can be expected that partners strive for balanced partnership triplets by either arranging a friendship tie between the partner and the friend or weakening or dropping the imbalanced friendship relation. From these observations, we derive two hypotheses:

Hypothesis 1. *People have a preference to close and maintain partnership triplets (figure 1(b)); they are likely to be friends with friends of their partner and to be friends with their friends' partners.*

Hypothesis 2. *The preference to close and maintain partnership triplets is stronger than the preference to close and maintain friendship triplets (figure 1(a)).*

Parks, Stan, and Eggert (1983, 118) state: “communication with the partner’s network might preserve ... romantic involvement by reducing the amount of time available for the pursuit of alternative partner.” Hence, triadic closure can additionally serve as a control mechanism to protect the partnership tie. Because the partnership tie is particularly valuable due to its intimacy, situations that may harm it will be avoided. Imagine that A and B are partners and A is friends with C, who is a potential new partner of A. If B is not friends with C—and therefore is not in a “controlling” position—then this situation may be perceived as imbalanced and stressful for B. Assuming that different gender is a necessary precondition of a potential romantic relationship,⁵ we call such an imbalanced triplet an *open mixed-gender triplet*. This structure is shown in figure 1(c). The imbalanced situation in this triplet can be avoided in different ways. Actor A, who is interested in the stability of the partner tie, may not maintain the friendship tie with C in the first place. Alternatively, B may either influence A to drop the friendship relation or become friends with C as well, to be in a controlling position. Therefore, open mixed-gender partnership triplets are expected to be rare structures; they are unlikely to emerge and are closed quickly.

Hypothesis 3. *People avoid friendship relations with others of the opposite sex with whom their partner is not friends (open mixed-gender triplets; see figure 1(c)).*

We furthermore expect the emergence of dual friendship relations between two couples. The resulting structure is called a *partnership quadruplet* and is shown in figure 1(d). These structures may emerge similarly to opportunity-related transitive closure and as a byproduct of shared preferences. We do not expect, however, partnership quadruplets in which the friendship relations are between individuals of different genders. Such a structure would incorporate two open mixed-gender triplets. These structures are assumed to be highly imbalanced following the arguments of the open mixed-gender triplet.

Hypothesis 4. *People have a preference to close and maintain partnership quadruplets (figure 1(d)); they are likely to be friends with the partner of their partner’s friend.*

Hypothesis 5. *People avoid being embedded in partnership quadruplets in which the two friendship relations are mixed-gender ties.*

We identified a number of mechanisms that lead to clustering of friendship networks around partnership ties. This clustering can amplify the level of homophily in the network. The homogamous partnership tie plays a critical role.

Imagine that both A and B and C and D are connected with two partnership ties. Religious homogamy may result in each of the four actors having the same

religion as his or her partner. If, additionally, both couples have the same religion (for example, they are all Catholic), religious homophily in A's friendship preferences may drive an initial friendship tie from A to C. Three subsequent friendship ties (B to C, A to D, B to D) may then partly be explained by preferences for partnership triplets and quadruplets (hypotheses 1 and 4) and may be independent of religious-homophilic choices. Eventually, four friendship ties between Catholic individuals are observed, but only the creation of the first one was solely based on *choice homophily*. The apparent homophily of the other three ties is *induced* (McPherson and Smith-Lovin 1987, 371), which we understand as the level of homophily that is above a population expectation without originating from homophilic choices. Schaefer (2012) describes a similar induction of homophily through non-reciprocity. Individual preferences are not exactly additive, as suggested in the example about homophily amplification above. The reason is that each individual will maintain only a limited number of friendship ties.

Hypothesis 6. *If partnership-related clustering mechanisms are prevalent, homophily will be amplified by clustering around homogamous partnership ties.*

In friendship networks of heterosexual couples, we may observe the opposite effect regarding gender homophily. Partner clustering mechanisms may *de-amplify* the level of gender homophily. In heterosexual partnership triplets and partnership quadruplets, half of the friendship ties are between individuals of opposite genders, even if the initial gender choice homophily is high.

Hypothesis 7. *If partnership-related clustering mechanisms are prevalent, gender homophily will be de-amplified by clustering around heterosexual partnership ties.*

Study Design

The role of partnership ties in the evolution of friendship networks and the interplay with different dynamic social forces—homogamy, homophily, and different types of clustering around partnership ties—have not yet been studied. The interplay of the mentioned mechanisms is dynamic, multi-mechanistic, and complex in nature. An empirical must consider all of these aspects. It is essential to study longitudinal data in complete social networks. The stepwise formation of the partnership quadruplet and other local clustering effects, for example, cannot be analyzed using ego network data (because information on some ties in quadruplets and other structures would be missing) or static network data (because this does not allow one to model different processes that lead to similar static outcomes). Finally, methodological tools for dynamic social network analysis that are tailored to multi-mechanistic problems have become available only recently and still need to be extended for specific research questions such as those presented in this paper.

In this study, we follow the evolution of self-reported friendship ties between 126 individuals who are all in a cohabiting and long-lasting partnership.

Approximately half already have children (49.6 percent). The data stem from a mobile-phone-centered data collection and are used for the first time to examine partnership ties. Friendship network data were collected with an online questionnaire in which each participant was asked to evaluate her relationship with each other participant in the network study. There were no restrictions regarding the number of friendship nominations. The participants in our study lived in a graduate housing community for married and long-term-cohabiting couples of a US university. The study spanned more than nine months starting in September 2010. Of the study population, 29.2 percent had moved to the location immediately before or shortly after the study began. Another 15.4 percent had moved earlier in 2010, and 19.2 percent and 23.1 percent, respectively, had moved in 2009 and 2008. Approximately 13 percent of the participants had moved earlier than this.

Participants often moved from other cities and countries after one of the spouses started a research-related appointment or graduate studies. Friendship ties were partly established at the time when the study started, but due to the high turnover in the community, we observed a highly dynamic, emerging friendship network. During the study period, friendship was measured four times among a subset of 126 participants who participated in at least one of the four waves. Descriptive statistics of the four waves are shown in table 1. Measurements took place in the academic year 2010–2011 in September, December, March, and May. Response rates varied from 92.9 percent (September, 117 participants) to 72.2 percent (March, 91 participants). The resulting one-phase missings can partly be imputed with friendship ratings of a preceding period according to the procedure described in Ripley et al. (2011, sec. 4.7). We therefore have two-phase missing rates ranging from only 3.9 percent (first two waves) to 19.7 percent (last two waves), which corresponds to a two-phase response rate of 80.3 percent to 96.1 percent. The friendship network density (before imputation and dichotomized as described below) varies between 3.2 percent and 3.8 percent. A pairwise comparison of the networks results in Jaccard indices of 67.5 percent (wave 1–2), 70.8 percent (wave 2–3), and 75.4 percent (wave 3–4). The Jaccard index is defined as the ratio of ties observed in both waves divided by the number of ties observed in either or both waves.

Table 1. Waves of the Friendship Network (126 participants)

	Wave 1	Wave 2	Wave 3	Wave 4
Month	09/2010	12/2010	03/2011	05/2011
Participants	117	104	91	94
Friendship ties	562	571	511	606
One-phase missings	7.1%	17.3%	27.6%	25.2%
Two-phase missings	3.9%	9.4%	19.7%	
Density	3.6%	3.6%	3.2%	3.8%
Two-phase Jaccard index	67.5%	70.8%	75.4%	

Friendship was measured on a scale from 0 to 7 defined as follows:

- 0: I don't know this person
- 1: I know of this person
- 2: This person is an acquaintance
- 3: This person is a friend (low ranking)
- 4: This person is a friend (medium ranking)
- 5: This person is a friend (high ranking)
- 6: This person is a close friend
- 7: This person is family or as close to me as a family member

We transformed the responses into four binary friendship networks over time, with a tie being present if a value of 4 or more was chosen and no tie otherwise.⁶

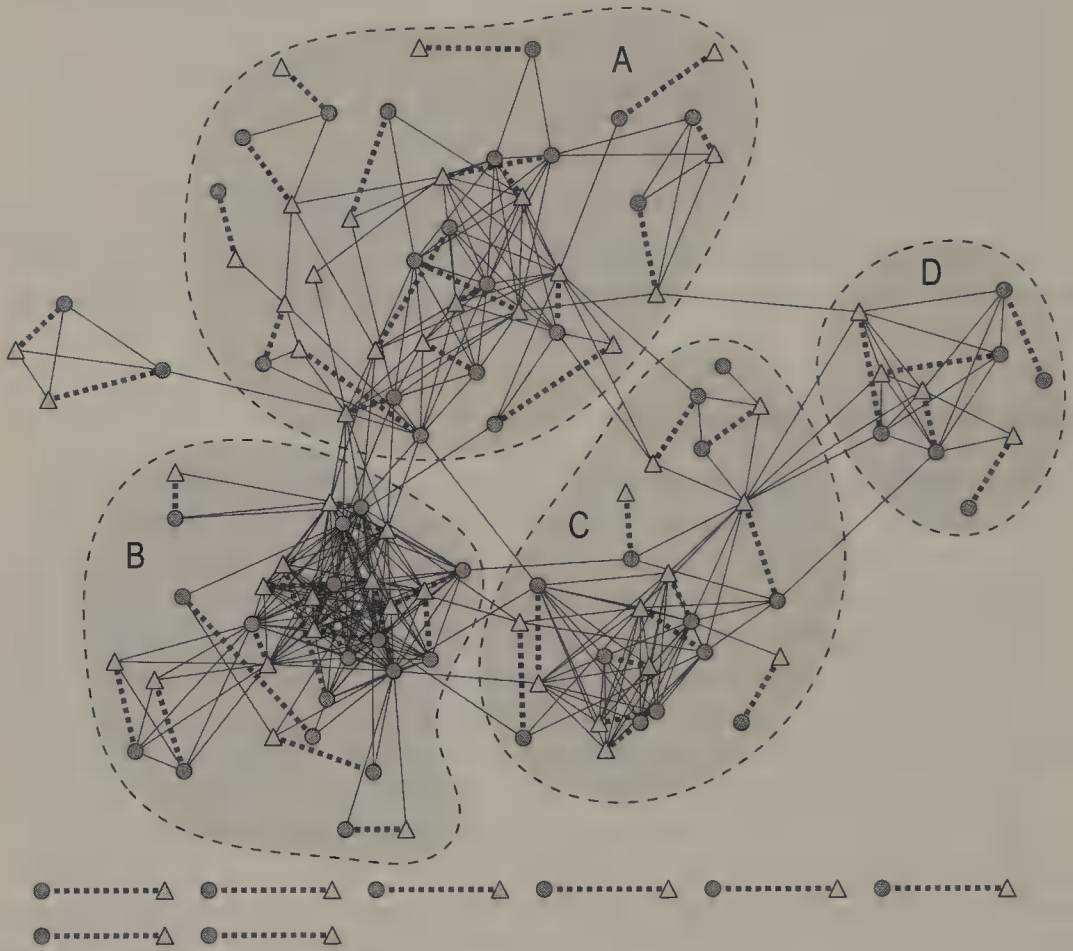
Static Network Descriptions

The study population is homogeneous regarding partnership statuses (100 percent are in a cohabiting partnership) and level of education (98.3 percent of non-missings have a university degree). The population is heterogeneous regarding age, gender, number of children, race/ethnicity, and religion. The participants are aged 22 to 42, with two outliers at the age of 54 and 60 (mean age: 28.6, standard deviation: 3.9 years, both without outliers). Of the participants, 48.4 percent are female and all but two couples are heterosexual. The five main ethnicities are (in alphabetical order) Asian (38.2 percent), Black (2.4 percent), Hispanic (8.9 percent), Middle Eastern (5.7 percent), and White (24.4 percent). Furthermore, 49.6 percent of the individuals have children. The seven most frequent religious denominations are (in alphabetical order) atheist or no religion (28.7 percent), Buddhist (5.7 percent), Catholic (11.5 percent), Hindu (3.2 percent), Jewish (12.3 percent), Mormon (16.4 percent), and other Christian denomination (20.5 percent).

Figure 2 presents a visualization of the friendship and partnership network in wave 1. Friendship ties are indicated by blue lines (they were symmetrized for the visualization), and partnership ties are indicated by dashed lines. Females are represented by triangles, and males by circles. Community structure (dense areas with sparse connection to the rest of the network) was discovered based on the modularity criterion with a fast-greedy algorithm (Clauset, Newman, and Moore 2004). These clusters are marked by gray areas with a dashed border. By comparing the attribute compositions of the clusters, we obtain an initial understanding of how homophily may interplay with the clustering of the network. Four clusters have 10 or more nodes, named A, B, C, and D. Other clusters are one of four persons on the left and eight couples below that do not have friendship relations with other couples in wave 1.

The mean age of the clusters is very similar, ranging from 28.1 (cluster A) to 30.4 years (cluster C). There is a bit more variation in the gender variable, ranging from 40 percent females in cluster D to 51.3 percent females in cluster A. These differences stem from two partners not participating in the friendship survey and two homosexual couples in clusters B and D. We find significant

Figure 2. The friendship network in wave 1: solid lines indicate friendship; dashed lines, partnership. Eight couples are not embedded in the main component of the friendship network in wave 1. Circles are male participants, and triangles are female participants. Clusters are indicated by dashed lines.



differences between the four clusters on the three other variables of race/ethnicity, having children, and religion. The main race/ethnicity is Asian (89.4 percent) in cluster A; White (78.1 percent) in cluster B; White, Asian, and Middle Eastern (43.4, 39.1, and 13 percent, respectively) in cluster C; and Hispanic (100 percent) in cluster D. Regarding parental status (having children, yes or no), cluster B (84.4 percent have children) differs significantly from clusters A (34.2 percent) and C (44 percent). The distributions of religion are clearly different. The main religious denominations are none (64.1 percent) and Buddhist (10.3 percent) in cluster A; Mormon (62.5 percent) and other Christian denominations (21.9 percent) in cluster B; Jewish (54.5 percent), other Christian denominations (22.7 percent), and Hindu (9.1 percent) in cluster C; and Catholic (70 percent) and none (30 percent) in the “Hispanic” cluster D.

Table 2 compares the average differences between partners, friends, and all individuals regarding the five attributes of age, gender, race/ethnicity, having children, and religion. Except for gender, partners tend to be significantly more similar than both reference groups. Almost all couples are heterosexual. Friends are, on average, more similar to each other than random individuals on all five

Table 2. Average Similarity of Partners (Homogamy) Compared with Average Similarity of Friends and All Individuals in Wave 1

Attribute	Partners	Friends (W1)	Random pairs
Age diff. (years)	2.0	3.1	5.1
Same gender	3.2%	54.6%	49.7%
Same race/ethnicity	78.3%	75.4%	32.4%
Same parental status	96.7%	68.2%	49.6%
Same religion	84.7%	75.3%	16.3%

attributes. This finding can be interpreted as initial, static evidence for homophily in the friendship network.

Dynamic Method and Model Specifications

In this section, we present a set of models to test the dynamic hypotheses 1–7 from above. We specify stochastic actor-oriented models for longitudinal network data (SAOMs; see Snijders, van de Bunt, and Steglich 2010), which are estimated with the RSiena software version 4 (Ripley, Snijders, and Lopez 2011). The models assume that individuals “optimize” their local friendship network configuration over time by changing the composition of outgoing ties according to dynamic preferences, such as homophily or transitivity. Individuals’ choices of whether to create, dissolve, or maintain a friendship tie over time are made “on the basis of their and others’ attributes, their position in the network, and their perceptions about the rest of the network” (Snijders, van de Bunt, and Steglich 2010, 6).

Differences between the four observed friendship networks indicate that individuals reconsider their friendship network configurations over time and create, maintain, and drop friendship relations with others.

Whenever an actor considers changing her set of outgoing ties⁷ (her personal network configuration), she is assumed to evaluate all possible outcomes of any of the following actions: keeping the network unchanged, nominating a new person as a friend, or dropping an existing friendship relation. This choice is formally modeled as a discrete multinomial choice model (McFadden 1974). The probability of actor *i* adding a tie to an actor *j* is, for example, expressed as

$$\begin{aligned}
P(i \rightarrow j; x, \beta) &= \frac{\exp(\beta^T s(i, x^{i \rightarrow j}))}{D_1 + D_2 + D_3} \\
D_1 &= \sum_{k \in A_i^-} \exp(\beta^T s(i, x^{i \rightarrow k})) \\
D_2 &= \sum_{l \in A_i^+} \exp(\beta^T s(i, x^{i \rightarrow l})) \\
D_3 &= \exp(\beta^T s(i, x)),
\end{aligned}
\tag{1}$$

where x is the friendship network before any change is applied, $x^{i \rightarrow j}$ if a tie $i \rightarrow j$ is added, and if $x^{i \rightarrow k}$ a tie $i \rightarrow k$ is removed from x . A_i^{\rightarrow} is the set of all actors that i is friends with before the decision is made, A_i^{\leftarrow} are those actors she is not friends with ($j \in A_i^{\leftarrow}$), and $A = \{A_i^{\rightarrow} \cup A_i^{\leftarrow} \cup i\}$ is the set of all actors. D_1 , D_2 , and D_3 are the parts of the denominator that evaluate the possibilities of i adding a tie, removing a tie, or keeping the network unchanged, respectively.

The probability of actor i dropping a tie to actor h and the probability of actor i keeping her personal network unchanged are expressed in a similar way:

$$P(i \rightarrow h; x, \beta) = \frac{\exp(\beta^T s(i, x^{i \rightarrow h}))}{D_1 + D_2 + D_3} \tag{2}$$

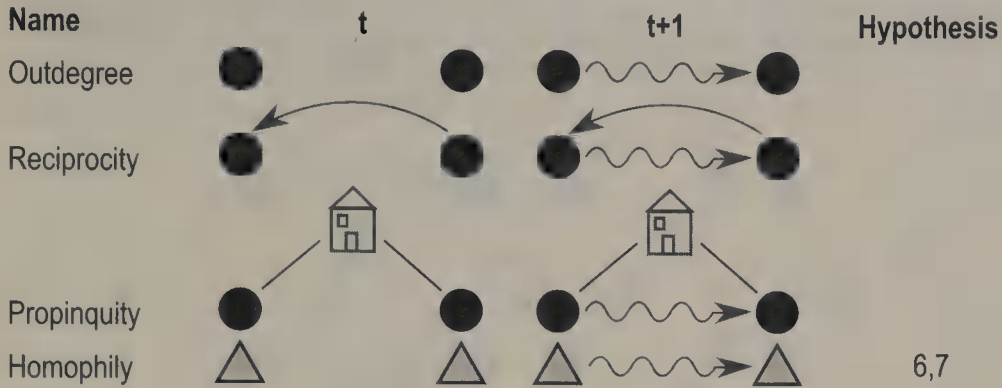
$$P(i \text{ no change}; x, \beta) = \frac{\exp(\beta^T s(i, x))}{D_1 + D_2 + D_3}. \tag{3}$$

The probabilities share a vector $\beta = (\beta_1, \dots, \beta_k)$ that weights a vector of choice statistics $s(i, x) = (s_1(i, x), \dots, s_k(i, x))$. These choice statistics $s(i, x)$ are operationalizations of dynamic preferences, such as the number of homophilic friendship ties that an individual maintains or the number of transitive structures in which she participates. The linear function $\beta^T s(i, x)$ can be interpreted as an objective function of an individual's personal network that she wants to maximize based on her preferences and the given opportunities in the network x . In the results section, we report the estimated parameter vectors $\hat{\beta}$ for models with different sets of choice statistics.

In our study, we test seven different models with 16 choice statistics that are grouped into five classes: dyadic effects, higher-order structural effects, homophily effects, triadic partnership effects (partner triplets), and effects involving two couples (partner quadruplets). In the following, we present these classes of effects, going from straightforward to complex effects and from friendship-endogenous to attribute-related to partnership-related effects. At the end of each section, we discuss their relationship with the seven hypotheses.

Dyadic Effects

As three baseline effects in the friendship formation process, we test the outdegree (number of friends nominated per actor), reciprocity (number of reciprocated friendship ties of an actor), and propinquity (tendency to be friends with someone who is a neighbor in the housing community). The families in our study are assigned to apartments by the housing administration. We construct a binary variable: neighbors are defined as those who are living in the same multi-family house or on the same floor of a large apartment building. The three dyadic baseline effects are shown in the first three rows of figure 3. The effect statistics $s_1(i, x) - s_3(i, x)$ count the number of outgoing ties, reciprocated ties, and friends nominated who are also neighbors. Formulas can be found in Ripley, Snijders, and Lopez (2011, 100, 104, no. 1, 2, 32).

Figure 3. Dyadic effects (including an exemplary homophily effect).

Higher-Order Structural Effects

We further control for a number of higher-order structural mechanisms by including four effects. First, *transitive triplets* counts the number of transitive friendship structures in which an actor is embedded, following the idea of “friends of a friend become a friend.” Second, we test the effect of circular (non-hierarchical) closure (*three-cycles* effect) in local environments. The third effect counts the number of actors to which the focal actor is only indirectly tied (*distance two* effect). This effect is closely related to transitivity. A negative parameter would indicate that actors tend to befriend friends of friends rather than maintaining open two-paths. Fourth, we test whether individuals with many friendship nominations are more likely to be further nominated (*in-degree popularity* effect). This effect can be interpreted as “preferential attachment” (Snijders, van de Bunt, and Steglich, 2010, 48). All higher-order structural effects are depicted in figure 4. The effect statistics $s_4(i, x) - s_7(i, x)$ count the number of transitive triplets, circular triplets, actors at distance two, and the sum of in-degrees of all nominated friends. Formulas can be found in Ripley, Snijders, and Lopez (2011, 100–102, no. 3, 5, 11, 15).

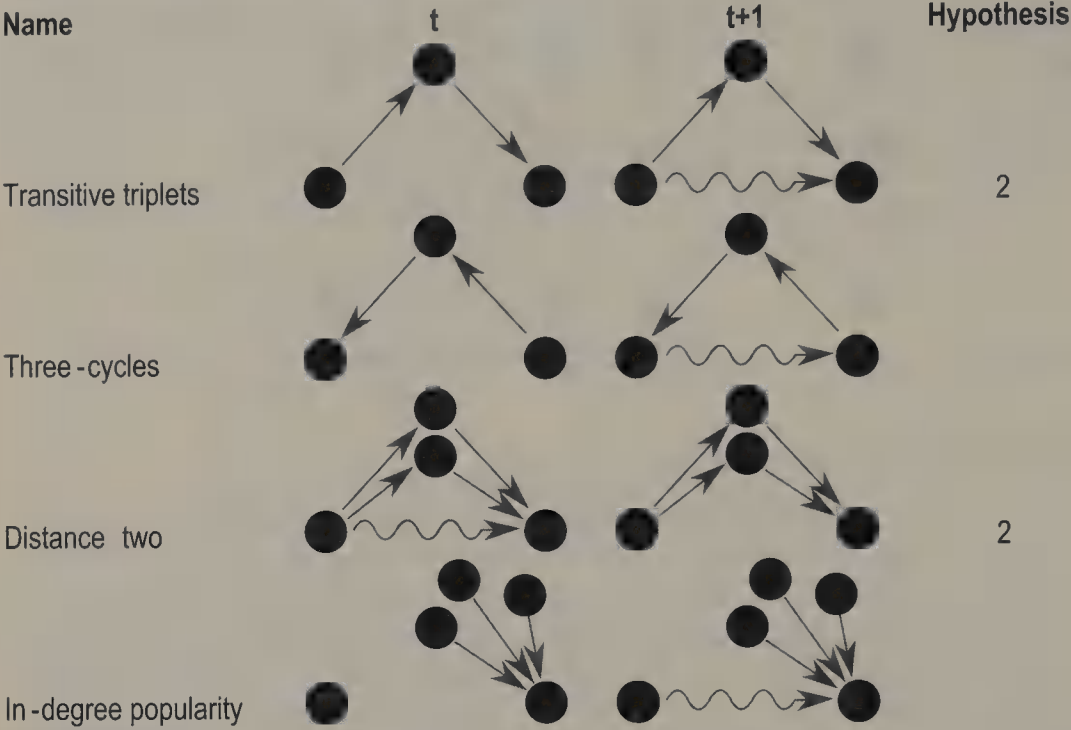
Hypothesis 2 states that we expect the tendency toward partnership triplets to be stronger than the tendency toward friendship triplets. Therefore, the corresponding parameters are expected to be larger than a combination of the transitive triplet effect and the distance two effect.

Homophily Effects

We test five different types of homophily: gender homophily, age homophily, homophily regarding race/ethnicity, parental status homophily, and religious homophily. Exemplarily, the dynamic process of gender homophily is depicted in the fourth row of figure 3 for a tie emerging between two female actors. The five effect statistics are called $s_8(i, x) - s_{12}(i, x)$. The formulas can be found in Ripley, Snijders, and Lopez (2011, 150, no. 40, 42).

Hypothesis 6 states that the level of homophily of homogamous attributes is amplified by partnership clustering. Therefore, we expect that the level of homophily in these attributes (age, race/ethnicity, parental status, religion) significantly decreases after controlling for the corresponding partnership effects.

Figure 4. Higher-order structural effects (structural).



Hypothesis 7 states that gender homophily may be de-amplified by partnership clustering. We expect a significant increase of the gender homophily effect after controlling for the corresponding partnership effects.

Triadic Partnership Effects

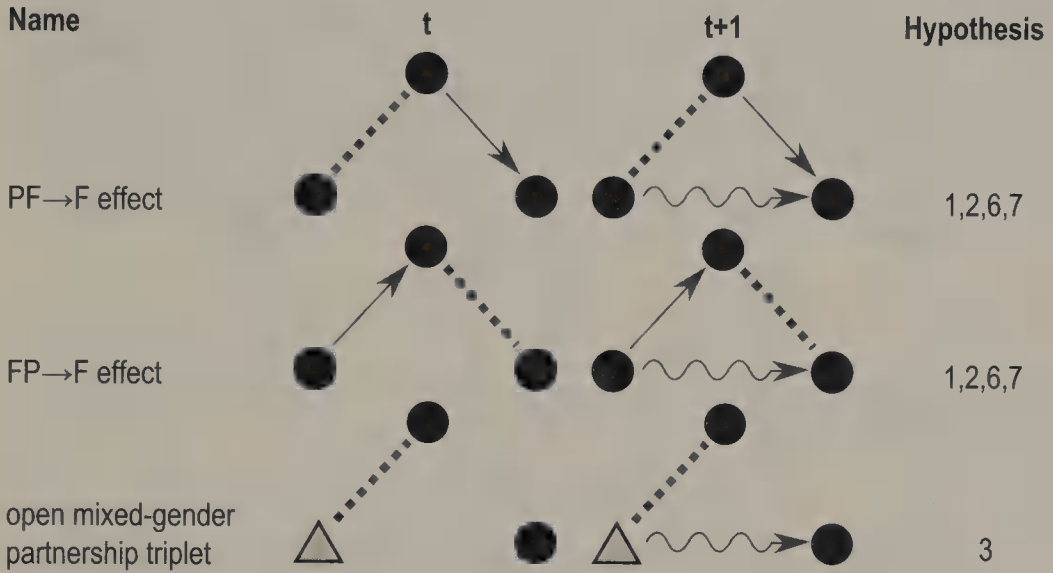
We test whether there is a tendency to form *partnership triplets*. Are friends (F) of a partner (P) more likely to become one’s friend (PF→F effect), and is the partner of one’s friend more likely to become a friend (FP→F effect)? Both effects are shown in figure 5. The third effect in figure 5 is what we introduced as an *open mixed-gender triplet*. It tests the tendency to be friends with someone of a different gender without the partner being involved. We refer to this group of three effects as *partner triplet* effects in the following. The mathematical definitions of the first two effect statistics $s_{13}(i, x)$ and $s_{14}(i, x)$ can be found in Ripley, Snijders, and Lopez (2011, 104–5, no. 35, 36). The open mixed-gender triplet effect is newly developed for this paper and is defined as

$$s_{15}(i, x) = \sum_{k, j \in A \setminus \{i\}} x_{ij} w_{ik} (1 - x_{kl}) I\{v_i \neq v_j\}, k \neq j. \tag{4}$$

The friendship network is denoted by x . Variable w_{ik} equals one if actor i and k are partners and zero otherwise. The indicator function $I\{v_i \neq v_j\}$ equals one if actor i and j have different gender attributes v_i and v_j (female = 1, male = 0). Otherwise, function I equals zero.

Hypothesis 1 states that we expect a tendency for closure of partnership triplets that should manifest in a positive effect. Hypothesis 2 states that this effect is

Figure 5. Examples of triadic clustering with ■ partnership tie, which is indicated as the dashed tie (partnership triplet). Friendship ties are solid arcs. A tie between a triangle and a circle is a mixed-gender tie. Ties between circles can be either same- or mixed-gender ties. The P in the effect names refers to ■ partnership tie, whereas the F refers to a friendship tie.



expected to be stronger (in absolute terms) than the tendency toward friendship transitivity (see above). Hypothesis 3 states that open mixed-gender triplets are unlikely structures. We expect a negative effect. Hypotheses 6 and 7 relate to the interplay between partnership clustering and homophily and have been discussed above.

Partnership Quadruple Effects

The final class of effects is related to clustering in the friendship network involving two couples (*PFP→F effects*). The first effect tests whether two individuals are more likely to nominate each other as friends if there is an existing friendship relation between their partners. We use this as a main effect and additionally test an interaction with gender similarity. Both effects are shown in figure 6.

Because we implemented the effects newly for this study, we also report the effect statistics:

$$s_{16}(i, x) = \sum_{k, l, j \in A \setminus \{i\}} x_{ij} w_{ik} x_{kl} w_{lj} I\{v_i \neq v_j\}, \quad (5)$$

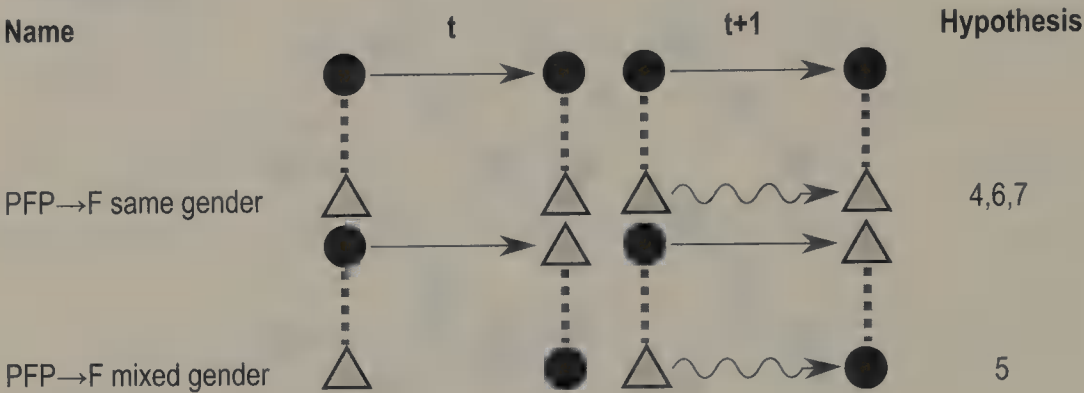
$$s_{17}(i, x) = \sum_{k, l, j \in A \setminus \{i\}} x_{ij} w_{ik} x_{kl} w_{lj} I\{v_i = v_j\}, \quad (6)$$

k, l, j are different.

The notation is as described below equation 5.

Hypothesis 4 states that partnership quadruplets are likely to emerge. Therefore, we expect a positive estimate of the *PFP→F same gender* effect. Hypothesis 5 states that a mixed-gender partnership quadruplet is an unlikely structure. We

Figure 6. Clustering involving two couples. Partnership ties are indicated as dashed lines (partnership quadruplet). Friendship ties are solid arcs. The focal actor is on the lower left, and the chosen actor is on the lower right. Gender similarity is indicated by ■ tie between triangles. The P in the effect names refers to a partnership tie, whereas the F refers to a friendship tie.



expect a negative estimate of the *PFP→F mixed-gender* effect. Both effects further relate to hypotheses 6 and 7 about the amplification and de-amplification of homophily.

Specifications

Above, we introduced five sets of parameters of the dynamic friendship formation process that operationalize the hypotheses discussed earlier as well as additional control mechanisms. To investigate the interplay of these parameter classes, we specify and estimate seven models in which the parameter groups are stepwise exchanged, starting with straightforward baseline models and ending with a fully specified model. An overview is given in table 3.

Results

Seven dynamic network models with different sets of parameter specifications were estimated. The results are shown in table 4. Each model consists of two columns showing parameter estimates and standard errors. A comparison of estimates between different models can reveal the interplay of parameters. Rate parameters (see Snijders, van de Bunt, and Steglich 2010) are reported but not discussed.

Dyadic effects are included in all models. *Reciprocity* and *propinquity* are significantly positive in most models, indicating the tendency of individuals to reciprocate friendship nominations and to nominate those as friends who are spatial neighbors. The probability of nominating a neighbor as a friend (propinquity) is 34 percent⁸ higher compared with non-neighbors. The odds of reciprocating a tie compared with non-reciprocation are 3.7 (model 7: $e^{1.31} = 3.7$). The *outdegree* effect is negative, indicating that the number of friendship nominations per actor is limited. It can be interpreted as an intercept because it counterbalances

Table 3. Seven Models (Model 1 – Model 7) Specified with Five Different Classes of Parameters (rate parameters are estimated in all models)

	M1	M2	M3	M4	M5	M6	M7
Dyadic effects	X	X	X	X	X	X	X
Structural effects	X		X	X		X	X
Homophily		X	X		X	X	X
Partner triplet				X	X	X	X
Partner quadruplet							X

against the rest of the model. The directions of these effects remain stable in all models.

The higher-order *structural effects* are significant in most models. *Transitive triplets* is significantly positive in all models, whereas *three-cycles* is negative.⁹ In combination with the negative effect for *distance two* structures (avoidance of open two-paths), we infer that there is a strong tendency for triadic clustering in the friendship network: individuals tend to be friends with their friends' friends. Additionally, the *in-degree popularity* effect is positive, which indicates that individuals with a high in-degree tend to attract and keep additional friendship nominations.

Homophily parameters clearly interplay with other parameters. Depending on the other effects in the model, homophily parameters are significantly positive or not. Further, there are remarkable changes in absolute values between the different models, even though we shall only carefully interpret these absolute changes, given that parameter comparisons are critical in log-linear models. Model 2 can be understood as a dyadic baseline model indicating whether homophily is generally prevalent in the friendship network. Positive parameter estimates indicate an overrepresentation of ties between similar individuals relative to the population expectation. A change of homophily estimates after the inclusion of parameters controlling for network clustering suggests the extent to which this homophily is *choice homophily* based on individual preferences and the extent to which it is *induced homophily* caused by other social mechanisms. We find that a large part of the overall homophily is induced by the amplifying effect of clustering, particularly the clustering around homogamous partnership ties.

For example, religious homophily has a significant estimate in the dyadic model 2 (0.73). After the inclusion of clustering and partner effects, however, we find no more evidence for religious choice homophily (the effect of 0.14 in model 7 is insignificant). This finding indicates that static observations of religious homophily are not solely based on choice homophily but, to a large extent, are induced by network clustering around religiously homogamous partnership ties.

Homophily in parental status is also amplified by network clustering around homogamous partnership ties. As with religious homophily, we find that the significant parameters of model 2 are eliminated in model 7, in which we control for all clustering effects.

Table 4. Estimates of Seven SIENA Models, Specified with Parameters Out of Six Classes (* $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$)

Parameter	Model 1		Model 2		Model 3		Model 4		Model 5		Model 6		Model 7	
	est.	s.e.	est.	s.e.	est.	s.e.	est.	s.e.	est.	s.e.	est.	s.e.	est.	s.e.
Rates														
Period 1	5.19	(0.49)	4.41	(0.38)	5.36	(0.49)	11.51	(1.72)	10.59	(1.54)	11.45	(1.70)	11.38	(1.60)
Period 2	4.19	(0.40)	3.84	(0.35)	4.40	(0.42)	7.59	(1.00)	7.07	(0.89)	7.69	(1.01)	7.89	(1.03)
Period 3	3.69	(0.36)	3.31	(0.30)	3.81	(0.37)	6.12	(0.78)	5.66	(0.70)	6.21	(0.80)	6.38	(0.83)
Dyadic effects														
Outdegree	-2.53***	(0.08)	-2.89	(0.09)	-3.05***	(0.11)	-2.64***	(0.07)	-2.75***	(0.07)	-3.13***	(0.08)	-3.06***	(0.08)
Reciprocity	2.49***	(0.15)	2.17***	(0.12)	2.22***	(0.14)	1.48***	(0.15)	1.10***	(0.13)	1.32***	(0.14)	1.31***	(0.14)
Propinquity	0.53***	(0.14)	0.54***	(0.13)	0.62***	(0.13)	0.26*	(0.13)	0.24	(0.13)	0.29*	(0.13)	0.29*	(0.12)
Structural effects														
Transitive triplets	0.12*	(0.06)			0.11*	(0.05)	0.11**	(0.04)			0.10**	(0.04)	0.10**	(0.03)
Three cycles	-0.17	(0.11)			-0.20*	(0.09)	-0.19**	(0.07)			-0.20**	(0.06)	-0.19**	(0.06)
Distance two	-0.24***	(0.03)			-0.21***	(0.03)	-0.08*	(0.03)			-0.07*	(0.03)	-0.06	(0.03)
In-degree popularity	0.11***	(0.02)			0.11***	(0.01)	0.04**	(0.02)			0.04**	(0.02)	0.04*	(0.02)
Homophily														
Similar age			0.41	(0.23)	0.08	(0.22)			0.19	(0.20)	0.06	(0.21)	0.04	(0.21)
Same gender			0.12	(0.07)	0.19**	(0.07)			-0.15**	(0.06)	0.20***	(0.06)	0.01	(0.07)
Same race/ethnicity			0.71***	(0.08)	0.59***	(0.08)			0.46***	(0.07)	0.43***	(0.07)	0.40***	(0.07)
Same parental status			0.29***	(0.08)	0.13	(0.07)			0.16*	(0.07)	0.12	(0.07)	0.11	(0.07)
Same religion			0.73***	(0.08)	0.39***	(0.09)			0.29***	(0.08)	0.17*	(0.09)	0.14	(0.09)
Partner triplet														
PF→F							0.74***	(0.14)	0.68***	(0.12)	0.76***	(0.12)	1.37***	(0.19)
FP→F							1.39***	(0.07)	1.52***	(0.07)	1.36***	(0.07)	1.52***	(0.09)

open mixed-gender	-0.98***	(0.13)	-1.18***	(0.08)	-0.76***	(0.08)	-0.82***	(0.10)
Partner quadruplet								
PFP→F mixed gend.							-1.62***	(0.33)
PFP→F same gend.							1.69***	(0.27)

There is only weak evidence for age homophily in our data. In the dyadic model 2, we find a borderline significant effect that is, however, eliminated with the inclusion of clustering effects.

We observe a contrary effect with gender homophily. Only after controlling for clustering (models 3 and 6) do we find strong evidence for choice homophily of gender in the friendship network. In the complete model 7, gender homophily is again eliminated, as same-gender preferences are sufficiently explained by the gender-related partnership clustering effects. These effects in combination suggest that, first, partners tend to agree on friendship choices and to befriend other couples. The initial friendship tie, however, tends to be a same-gender tie. Second, individuals avoid mixed-gender friendship relations, as suggested by the effects related to the open mixed-gender triplets and mixed-gender quadruplets. This means that gender choice homophily is prevalent (even though there is no evidence in the dyadic baseline model), but it is partly driven by an *avoidance* of imbalanced mixed-gender ties. In the dyadic baseline model 2, we do not find statistically significant evidence for gender homophily at all because it is suppressed by the many mixed-gender ties induced by clustering around partnership ties. In model 5, we even find a significantly negative gender homophily effect which, however, is a side effect of an incomplete model specification¹⁰ that does not represent clustering very well and focuses only on partly gender-related triadic partnership effects.

Evidence for ethnic choice homophily is prevalent in all models. Net of all other effects, a tie between actors of the same race/ethnicity is 49 percent more likely to be formed than a mixed-ethnic tie (model 7: $e^{0.40} = 1.49$). The probabilities in the dyadic baseline model 2 are higher (a mixed-gender tie is 103 percent more likely: $e^{0.71} = 2.03$). However, we refrain from interpreting these absolute differences, as this is critical in log-linear models. Nevertheless, we may loosely argue that a comparison of the log odds indicates that in the case of ethnicity, the partnership cluster mechanisms also explain a relevant proportion of the observed static homophily.

We find the *partner triplet effects* to be significant in all models. Both types of partner triplets (PF→F and FP→F) are positive: individuals tend to nominate friends of their partners as friends and the partners of their friends as friends. The tendency to close partnership triplets is more than three times stronger than the tendency to close transitive friendship triplets.¹¹ The parameter relating to the *open mixed-gender triplets* is negative. We infer that individuals avoid nominating friends of different genders as long as the partner does not maintain a “confirming” friendship. If mixed-gender ties are created, this is only after the partner has become friends with the other person. The nomination of a

different-gender friend is 8.9 times more likely if there is “approval” by the partner (PF→F), indicated by a friendship tie (model 7: $e^{1.37}/e^{-0.82} = 8.9$).

The two *partner quadruplet* effects in model 7 are both significant. Net of partnership triplets, we observe a tendency for individuals to nominate same-gender friends if their partner is friends with the new friend’s partner. This leads to a situation in which the two men and two women of two heterosexual couples are friends but no mixed-gender friendship ties exist. A similar pattern with mixed-gender friendships only, however, is highly unlikely. These two effects interplay with the PF→F partnership triplet. After the inclusion, the PF→F estimate almost doubles, whereas all other effects (except for gender homophily, as discussed) remain stable.

Discussion and Conclusion

Our study finds that partnership ties strongly influence the shape of friendship networks. We find striking support for our hypotheses of how friendship networks cluster around partnership ties and how this mechanism relates to homophily and homogamy. The very high level of ethnic and religious segregation within the friendship network of the community studied (see figure 2) is explained partly by choice homophily and partly by the amplifying effects of clustering around homogenous partnership ties.

We find strong evidence that individuals strive to close partnership triplets (hypothesis 1). In addition to transitive clustering (“friends of my friends become my friends”), individuals in our study tend to become friends with friends of a partner and with the partner of friends. This effect is approximately three to four times stronger than the closure of transitive triplets (hypothesis 2). This finding supports our argument that opportunity-related closure, closure as a byproduct of similar preferences, and closure based on cognitive imbalance are more likely when a partnership triplet is to be closed. These findings are in line with the theoretical considerations of Heider (1958) and Granovetter (1973).

In addition to the tendency to close imbalanced partnership triplets, we find strong evidence that individuals avoid creating mixed-gender friendship relations if their partner is not also friends with that person (hypothesis 3) and thus is in an approving position. We hypothesized that these “open mixed-gender triplets” are perceived as highly imbalanced by the partner of the friendship tie initiator. Individuals consider their partner’s perceptions when making choices about personal friendship networks. We argue that friendship ties may serve as a control mechanism to secure the stability of the intimate partnership tie, which partly explains the particular avoidance of open mixed-gender triplets. This idea is in line with the arguments of Parks, Stan, and Eggert (1983).

We hypothesized that couples befriend other couples: individuals are likely to be friends with the partner of their partner’s friends. The emerging structure is called a partnership quadruplet (hypothesis 4). The argument is based on opportunity-related interaction and the likelihood of similar preferences. We find strong evidence for the emergence of such structures with two partnership ties as long as the friendship ties within the quadruplets are between individuals of the same gender. Mixed-gender partner quadruplets are avoided (hypothesis 5) because

they incorporate similar structures as the open mixed-gender triplets. The formation of partnership quadruplets was expected based on an extension of the arguments about triadic closure by Granovetter (1973).

Partnership ties in our study are characterized by high homogeneity regarding race/ethnicity, religion, and age. Homogamy is a typical characteristic of partnership ties (Kalmijn 1998). Additionally, partners in our study mostly have common children and different genders. We argue that the high level of homogamy amplifies the level of homophily in the friendship network through its interplay with clustering around partnership ties (hypothesis 6). Indeed, we find strong evidence for this dynamic interplay in our study. By comparing models in which we selectively control for different types of partnership clustering, we are able to investigate the amplifying effect of these mechanisms. McPherson and Smith-Lovin (1987) discuss the difference between choice homophily (homophily based on homophilic preferences) and induced homophily. We find strong support for choice homophily in the race/ethnicity attribute.

However, the level of homophily is amplified by clustering around ethnically homogeneous partnership ties. We find no evidence for choice homophily in parenting, religion, and age in a fully specified model. However, potentially small preferences of individuals are significantly amplified through these clustering mechanisms, so we find flawed evidence for homophilic preferences when we do not control for partnership clustering. We conclude that a significant proportion of this homophily is potentially induced.

Gender homophily is a special case: we find strong evidence for gender choice homophily only *after* controlling for partnership-related clustering. The mainly heterosexual partnership ties in the community studied induce a large number of mixed-gender ties (hypothesis 7). If a heterosexual couple has a common friend, then one of the two friendship ties will be a mixed-gender relation. Only after including triadic partnership effects in our model do we find clear evidence of gender choice homophily. Interestingly, this gender homophily is eliminated when controlling for the two partnership quadruplets that *amplify* gender homophily: same-gender friendship relations lead to additional friendship ties between the two partners of the friends—a tie that is again a same-gender tie. Another part of gender homophily is explained by the *avoidance* of imbalanced mixed-gender structures (gender heterophobia). These findings are in line with and extend Kossinets and Watts (2009) and Goodreau, Kitts, and Morris (2009), who discuss the potential interplay of general network clustering and homophily.

Our study has certain limitations. First, we investigate the influence of partnership ties on friendship ties, but there is certainly also an influence of friendship ties on partnership ties. For example, friends may become partners or introduce potential partners. We do not investigate the effect of friendship on partnership ties because no partnership ties emerge or dissolve in our study. Second, we could not study the effect of choosing friends who are not in a partnership because only individuals in stable relationships participated in our study. However, having a few “single” friends in a personal social network may provide a type of social capital that friends who are in a relationship cannot provide. The amplifying effect of partnership ties on homophily may be weaker in a population consisting of singles and couples. The hypothesis of friendship agreement as

a matter of control might, however, be more relevant in a mixed single-couple context. Third, we have no information on friendship ties prior to moving into the graduate housing community. There are good reasons to assume that most couples do not know anyone else when joining the community because the cultural and geographical background of the tenants is very diverse. However, there may be previously existing friendship ties that induce homophily (e.g., by two befriended couples moving from a different country together). This situation would be misinterpreted as choice homophily in our dynamic results. Fourth, the community studied is atypical in other ways. Couples decided to live in a graduate community of partners in the first place (and to work in a university environment), so there are certainly self-selection effects in the community composition that might affect the generalizability of the results. Furthermore, all individuals are new to the place and therefore may have few local friendship ties. As such, the friendship network is likely to be more dynamic than an average friendship network. Further, the level of heterogeneity in religion and race/ethnicity is higher than in many other residential networks that were, for example, formed over several generations. However, we believe that these particularities of our study are a fascinating feature. They allow us to study a variety of concurrent partnership- and attribute-related network mechanisms in a relatively small environment for which we could collect unique complete, longitudinal social network data. The effects that we find are very clear, and we believe that they are generalizable to a certain degree. Finally, there are methodological limitations and assumptions in the SIENA framework (Snijders, van de Bunt, and Steglich 2010, section 2). We loosely interpret large changes of parameter estimates and changes of parameter significance between models as an indication that both choice homophily and induced homophily are present. However, we refrain from interpreting the absolute changes, as this is considered critical in log-linear models. Further, the different estimates of rate parameters make model comparisons in terms of log odds critical. However, because the model differences are quite unambiguous (e.g., highly significant versus insignificant estimates), we are confident that our approach of model comparison is valid. The SIENA framework is the most advanced statistical tool for the analysis of multi-mechanistic social networks hypotheses. It allowed us, for the first time, to test hypotheses on the influence of partnership ties on the dynamic formation of friendship networks. In particular, we could investigate the interplay of homophily, homogamy, and a variety of theoretically motivated partnership-related clustering mechanisms.

We show that partnership ties work as nuclei in the formation of clusters in the friendship networks. A high level of homogamy between partners amplifies the level of homophily regarding race/ethnicity, religion, and parental status. It is, however, not mainly individual preferences (choice homophily) that explain the formation of ties between similar individuals. Rather, weak homophilic preferences and a tendency to cluster around (mostly homogamous) partnership ties are the origin of highly “homophilic” clusters. In a dyadic baseline model, we find, for example, evidence of homophily in religion but no evidence of same-gender preferences. After controlling for partnership clustering, the effects swap: we find

no more evidence for religious choice homophily but significant effects for a preference for same-gender relations.

These findings imply that not controlling for partnership ties and their level of homogamy may result in an overestimation, incorrect reporting, or overlooking of homophily effects. Partnership ties are crucial in the formation of adult friendship networks. Although our study finds 6.5 times more friendship dyads than partnership ties, the partnership ties work as nuclei in the friendship formation process. Our research suggests that the effect of partnership ties should be considered when studying the evolution and shape of friendship networks.

Notes

1. Family ties, for example, are less based on individual choices.
2. We refer to stable partnership ties, such as marriage and long-term cohabiting partnerships.
3. The partner of two individuals did not participate in the network survey.
4. Granovetter argues from an undirected network perspective that does not explicitly capture the idea of individual choices.
5. For reasons of simplicity, we now consider the case of heterosexual actors by using gender differences as a proxy for potential romantic involvement.
6. We tested the robustness of this dichotomization threshold by additionally conducting all subsequent dynamic analyses on a friendship network in which a friendship tie was defined as present when a value of 5 (high-ranking friend) or more was chosen. In that case, the estimates related to the hypotheses change in absolute size, but the directions and significance of the effects are robust.
7. The continuous-time model of these change considerations is explained in Snijders, van de Bunt, and Steglich (2010).
8. Based on model 7: the increase of the objective function in case of a propinquity choice is 0.29×1 . The odds of this choice are $e^{0.29} = 1.34$ compared with a non-neighbor choice assuming all other effects constant.
9. A negative three-cycle parameter in combination with a positive transitivity parameter is a common finding in stochastic actor-oriented models and usually is interpreted as an indicator of either hierarchy in transitive relations or a lower relevance of reciprocation in small groups (Block 2015).
10. Because higher-order structural effects are missing, choices about ties within friendship triplets are assumed to be conditionally independent. In transitive friendship structures, the closing tie is—net of other effects such as the open partnership triplet in this particular model—empirically more often a mixed-gender tie. That leads to a false detection of heterophilic preferences.
11. We calculate this as follows: closure of one friendship triplet in model 7 increases the probability of a corresponding choice by times $e^{(0.10+0.06)} = 1.17$, as one triplet is created and the number of open two-paths is reduced by one ($-1 \cdot -0.06$). Closure of a PF→F triplet increases the likelihood of the corresponding choice by times $e^{1.37} = 3.94$. The odds of these probabilities are 3.35. If an individual has the choice between closing one friendship triplet or one PF→F partnership triplet, she would 3.35 times more often close the partnership triplet (3.9 times more often in case of the FP→F effect). The ratios are only a rough comparison because they assume two alternative choices that close exactly one triplet each. However, one new friendship tie may close multiple triadic structures but can close a maximum of one partnership triplet of each type.

About the Authors

Christoph Stadtfeld is Assistant Professor of Social Networks at ETH Zürich, Switzerland. He holds a PhD from Karlsruhe Institute of Technology and has been postdoctoral researcher and Marie-Curie fellow at the University of Groningen, the Social Network Analysis Research Center in Lugano, and the MIT Media Lab. His research focuses on the development and application of theories and methods for social network dynamics.

Professor Alex “Sandy” Pentland has helped create and direct MIT’s Media Lab, and advises the World Economic Forum, the UN Secretary General, and various Fortune 500 companies. He is among the most-cited computational scientists in the world, is a member of the US National Academies, and is a pioneer in computational social science and organizational engineering. His most recent book is *Social Physics*, published by Penguin Press.

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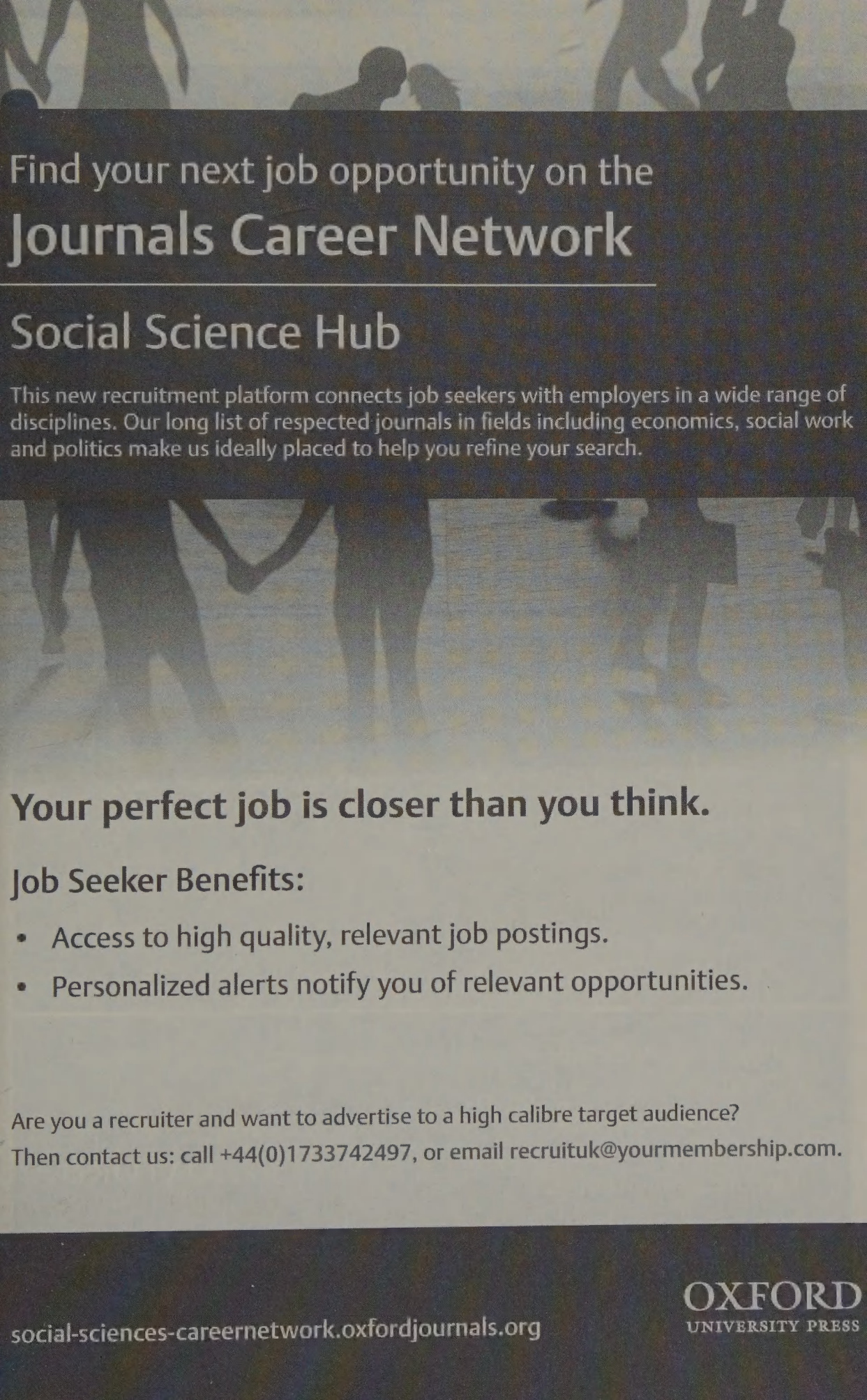
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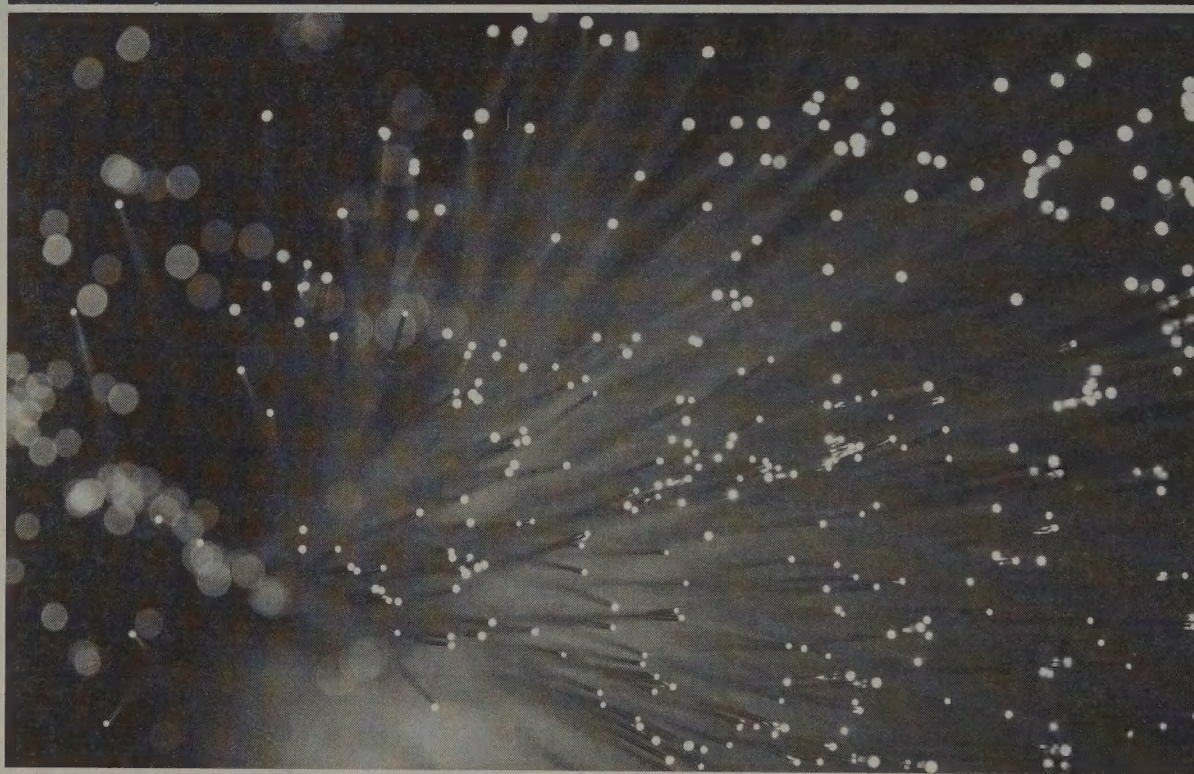
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